Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Reactor Safeguards Thermal-Hydraulic Phenomena Subcommittee
Docket Number:	(not applicable)
Location:	Rockville, Maryland
Date:	Tuesday, February 27, 2007
Work Order No.:	NRC-1437 Pages 1-374

NEAL R. GROSS AND CO., INC.
Court Reporters and Transcribers
1323 Rhode Island Avenue, N.W.
Washington, D.C. 20005
(202) 234-4433

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

+ + + + +

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

THERMAL HYDRAULIC PHENOMENA SUBCOMMITTEE

+ + + + +

TUESDAY,

FEBRUARY 27, 2007

+ + + + +

The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Sanjoy Banerjee, Chairman, presiding.

MEMBERS PRESENT:

SANJOY BANERJEE	Chairman
GRAHAM B. WALLIS	ACRS Member
THOMAS S. KRESS	ACRS Member
SAID ABDEL-KHALIK	ACRS Member

NRC STAFF PRESENT:
ERVIN GEIGER
TONY SHAW
ROB TRAGONING
PAUL KLEIN
MIKE SCOTT
PAULETTE TORRES
JOHN LEHNING
WILLIAM KROTIUK

ALSO PRESENT:
JOHN BUTLER
TOM MICHENER

A-G-E-N-D-A

P-R-O-C-E-E-D-I-N-G-S

(8:33 a.m.)

CHAIRMAN BANERJEE: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Thermal Hydraulic Phenomena. I'm Sanjoy Banerjee, Chairman of the Subcommittee. Subcommittee members in attendance are ACRS Members Graham Wallis, Tom Kress, Said Abdel-Khalik.

The purpose of this meeting today is to discuss the NRC Staff's progress involving research activities on chemical effects associated with the resolution of NUREG safety issue 191, PWR sump performance. The subcommittee will hear presentations by and hold discussions with the NRC staff and other interested persons regarding these matters. The subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the Full Committee. Ralph Caruso is the Designated Federal Official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the *Federal Register* on January 30th, 2007, and February 15 th,

2007. A transcript of the meeting is being kept, and will be made available, as stated in the *Federal Register* notice. It is requested that speakers first identify themselves, and speak with sufficient clarity and volume so that they can be readily heard. I would also like to remind the members that the committee has determined that speaker should be allowed the first 10 minutes of presentation time without question from the members.

MR. WALLIS: Was that ever really decided?

MR. KRESS: I don't remember voting on that.

MR. WALLIS: This is a Ralph Caruso inquisition.

MR. KRESS: Did we vote on that, or didn't we?

MR. CARUSO: We did, back a long time ago.

MR. KRESS: I think the P&P Subcommittee just imposed that.

MR. WALLIS: But it's never been implemented.

MR. CARUSO: It was tried several times.

CHAIRMAN BANERJEE: What do you think, should we, or shouldn't we?

MR. KRESS: I think we ought to play it by ear.

CHAIRMAN BANERJEE: All right.

MR. KRESS: If you have a burning question, you ought to ask.

CHAIRMAN BANERJEE: I think we'll play it be ear then. And I don't have any real comments to make, except that let's try to stick to the time, and I'll introduce Mr. Geiger of the staff to begin the presentation.

MR. GEIGER: My name is Ervin Geiger.

MR. KRESS: Do you have anything to do with the Geiger counter?

(Off the record comments.)

MR. GEIGER: I'm with the Office of Nuclear Regulatory Research. I usually talk with a soft voice. I'd just like to thank the subcommittee today for giving us the opportunity to present the final research or research activities. We're providing an overview of the research program. The program has been going for quite a long time, and we have completed the research projects that were originally slated, so I'd just like to present an overview of what we have accomplished so far, and go into some other presentation.

MR. WALLIS: You're going to speak about

all the projects?

MR. GEIGER: Well, I'm just going to -- I understand that this is a chemical effects, but I just thought I would brush up over some of the other issues.

MR. WALLIS: Are you going to speak about the Pacific Northwest experiments, for example?

MR. GEIGER: Yes.

MR. WALLIS: You will. Okay.

MR. GEIGER: Yes, this afternoon, because I was under the impression that the subcommittee was very interested in the Northwest Laboratory tests, and also Bill Krotiuk's correlation NUREG, so I thought it would be proper to present this at this time, since it hadn't really been discussed before in its details, since it was just completed at the end of this year, if the committee has no objection.

MR. XIAO: Erv, can I just interject for one thing?

MR. GEIGER: Right.

MR. XIAO: My name is Tony Xiao. I'm the Branch Chief of the Mechanical and Structural Engineering Branch in Research. It's under my branch this research work has been conducted for the past several years, and the purpose of today's meeting, as the Chairman described earlier, was really to summarize the work we have done so far. We have completed all the planned research work associated in support of Resolution GSI-191. And so Erv will provide the overview, and we have some change of personnel, as you recall. Dr. Rob Tragoning was the Program Manager for the past couple of years. He will join us shortly, but today - since then, Erv had taken over as the Program Manager for the overall project as we were winding down. As you will hear later from Mr. Geiger, we have published, or are about to publish a total of 15 NUREG reports and letter reports, and NUREG/CR reports. In the detailed discussion, as you will see from the agenda, will include the work, as Dr. Wallis referred to earlier, the PNNL work, and the head loss correlation by Bill Krotiuk in the afternoon. After Mr. Geiger's summary and overview, Ms. Torres will also describe in more detail the peer review process we have employed to ensure the quality of the research work. So this is overview as we're coming to the end of this research program, to wrap it all up, and to make a presentation to you before the Full Committee. Thank you.

CHAIRMAN BANERJEE: Now let me ask a question. The peer review, if I understand, was mainly on chemical effects.

MR. XIAO: Yes, it was. Yes.

CHAIRMAN BANERJEE: So is the focus of the discussions today going to be chemical effects, or is it going to be the whole program? It seems that Mr. Krotiuk will present correlations and so forth, which, perhaps, have nothing to do with chemical effects.

MR. GEIGER: Well, no it does not in that respect, no. The chemical effects research had been presented in previous subcommittees to a great extent, mostly include the ANL that was presented in great detail in past ACRS meetings, so I didn't feel that there was any additional information that we could really offer the subcommittee at this point. All the contracts had lapsed, they had been completed. There was no budget remaining to bring in any of the contractors to discuss any additional wrap-up issues that they might have, so what the object -- and since we had not really presented Bill Krotiuk's research, the PNNL research, and the head loss correlation, we thought this would be an opportunity to present that, and that would be this afternoon.

MR. WALLIS: Well, chemical effect we learned could be very big.

MR. GEIGER: Well, if --

MR. WALLIS: We also learned that we didn't have a quantitative way of predicting them. Are you going to tell us how you faced this sort of situation?

MR. GEIGER: Where we are right now with the research is that -- the intent of the research initially was to inform GSI-191 problem. We did some, as you know, the ICET test to see what kind of -- to prove to the industry that there was an issue with generating all those precipitates and chemical products that could effect sump. And then in conjunction with that, the ACRS recommended that we investigate the chemical effects research.

When we finished the ICET, and the ICET had no -- part of the ICET program did not include any testing of head loss; therefore, we went on did the ANL testing, we did some testing at PNNL, and at ANL, to determine what the effects were. Now at ANL, the tests we ran, we determined that even small quantities of these chemicals, aluminum oxyhdroxides caused a great deal of -- as soon as there's even a small amount precipitated, it created head loss across the screens, across the fiber bed that were really -- the pressure drops were excessive, and really exceeded the allowables at many of the plants, if not most of the plants. Okay? We know that, right? So where we are right now is -- and the industry, at the same time, had done similar tests, and they came up with the same conclusion. So at this point, what the industry is doing is they're re-evaluating their entire program on how they're going to assess or address the head loss issue across the --

MR. WALLIS: The real question would seem to be what is industry doing when you have found out there are large effects?

MR. GEIGER: Yes.

MR. WALLIS: You know you have to now be aware of that, but the real question is how do we determine whether or not they occur in plants, and if they do occur, how big they are? Those are questions that industry is presumably answering, so really we need presentations from those folks.

MR. GEIGER: Well, they are doing testing on that. Unfortunately -- well, Research is not in direct communication with the vendors; therefore, Research has no mechanism for actually going out and asking the industry as to how they are -- what they are doing. We've had meetings with not EPRI, but - I'm sorry - NEI, yes, thank you. We had meetings with NEI, we had many meetings with NEI. They have provided presentations where the industry is going. And, actually, I have some of that at the end of this presentation as to what the industry is doing to alleviate the problem with the chemical effects on the fiber bed.

At this point, we have no direction exactly which area we should research. The possibilities of researching all these different chemical effects, as outlined even in the peer review, the field is so great that we would be evaluating all of these different, from different concentrations, to different temperatures. We could be looking at this for a long period of time, so if we can narrow down the parameters that are of consideration, then we may have a better chance at getting the right answers, knowing what to do.

So I guess my answer is, this is not like a university research project where somebody wants to see what the effects of a certain chemical is on a certain thing.

MR. TRAGONING: Hey, Erv, I'm sorry. I think Paul could maybe add some information to address Dr. Wallis' comment, in terms of -- I'm sorry. This is Rob Tragoning from Office of Research. Paul Klein from NRR can add some more specific information in terms of what the industry is doing, and how they're approaching the problem, and sort of what our stance is with respect to evaluating the industry's -- their proposed approach.

CHAIRMAN BANERJEE: So can we have him address this at some point today? Do you want to do it now or later? Let's do it at the end of his talk.

MR. KLEIN: I guess maybe I'd add a few comments, at this point, if I could. I'm Paul Klein from NRR. Just to provide some broader perspective. Initially, the question was raised by ACRS about chemical effects. There was a joint NRC-Industry sponsored test that identified chemical products could form. And then the question became what do these precipitates mean in terms of head loss, so the NRC sponsored a number of tests to try and understand not only what they mean in terms of head loss, but also how parameters such as temperature, pH, and other important things that vary within a post LOCA pool could affect head loss, so we sponsored a number of tests that evaluated that parameters.

We certainly don't have all the answers at this point, but I think we identified a number of key parameters. And at the same time, our management made a decision that it really was at a point in understanding that industry needed to address the issue, so they are aggressively working the issue. They have a number of different strainer vendors that are conducting head loss tests. The staff observes a number of these tests. We visited the facilities, we have periodic meetings with industry about every other month where they provide us status update and describe some of their test results. They are working on a generic chemical model that would be used to try and predict what might form in the sump.

There's a number of activities underway. It's quite possible on the back end of this, once NRR has a better understanding of how industry is proceeding to try and resolve chemical effects. We might need independent research on the back end to confirm what's been observed in industry tests, and to help us evaluate the licensee submittals.

MR. TRAGONING: Rob Tragoning again from Research, if I could just add a little bit to what Paul stated. One of the things that we definitively learned is, I would agree with Paul, is we learned in many cases the effect of important variables, and we have some ideas of some cases where we saw very large effects due to chemical precipitants, and some where we saw either little or no effect. But one of the other things that we learned was that the plant-specific conditions are very important, in terms of not just the materials that are existing at individual plants, but also their containment pool conditions in terms of temperatures, pH, and, again, other materials that are within that sump pool environment. So really, when we see the individual submissions coming in, and we've stressed this to industry time and time again, that an understanding or a characterization of their unique mix, and their unique conditions is really important.

I think at the end of the day, when we see the submissions from industry, there'll be a certain percentage that we look at the conditions, and based on the research that we've already done, we'll be able to pretty clearly state well, there's probably not concerns. There will be other areas where we may have specific concerns that have been highlighted by the research that we've already done, and then there'll be other areas that we may have some gaps, or that we may not know if there are issues that need to be addressed for that specific set of plants that have a particular mix of materials and conditions that we may have some concerns about. And if that's the case, at that point, it would be particularly appropriate to embark on some possible additional confirmatory research to validate the work that the industry is doing.

CHAIRMAN BANERJEE: What is it that you would like the ACRS to do? I mean, this is a subcommittee meeting, and then we are going to go to a Full Committee meeting next week. What is the --

MR. GEIGER: Well, let me say that right now the industry is planning all of their tests, so they have not -- they have done some preliminary testing. They've come to the same conclusion that we have, that there are some serious issues that need to be resolved. Now, like Rob said, the different plants are pursuing different strategies, and the additional -- these strategies and tests will not be in place until this summer, so they really will not have any resolution to this until late into fall, probably, when they will know exactly what the modifications are, because they're putting in larger screens, and in some cases the larger screens may -- in addition to the larger screens, they may need to do other things, like remove the materials that are problematic.

Okay. The question posed as to what I would like the ACRS to do, at this point, I think my impression was that the ACRS was interested in our progress, and the status on what we have learned today in areas other than also just chemical effects. I guess if I misread the subcommittee's intent, I apologize.

MR. WALLIS: I guess we could talk a lot about this. I mean, you've told us that you stopped work, and if you went on, it would take you years to sort of really solve the problem. And then you've told us that each plant has a specific situation which has to be worked out. Now is that plant going to spend years working on that specific situation before it's got an answer?

MR. XIAO: Dr. Wallis, this is Tony Xiao again from Research. We would like to request that ACRS at the subcommittee today, as well as the Full Committee in March would look at what we have done so far up to this point. As I stated earlier, the purpose of this meeting and the March meeting is to present the overall view of the program we have completed so far. We have no plans to do any additional research unless the industry has done something like Paul and Rob alluded earlier, that we feel it's necessary to do additional confirmatory. At this point, we have no plans to do any more, so we would like to request that ACRS look at this whole package of the research and the reports we've done so far given the context of the condition and the timing we have, so that this is sufficient research for us to be able to support the regulatory decision in making sure the industry is doing this properly in an engineering manner that's appropriate to resolve these issues. As far as plant specific, that has to be seen, so to request ACRS just to write a letter to see what your review is as far as the research work done so far.

MR. WALLIS: We've already done that, and we're not going to learn much more today. It's been commented on a year or so ago.

MR. SCOTT: Graham, can I answer your question directly about the industry's intent? This is Mike Scott, NRR. I would say a couple of plants have largely completed their chemical testing. We know of at least one that states, and have provided us data to indicate that they are complete with chemical testing.

As was said, a lot of the rest of the chemical testing will go on this spring and summer, and the intent of the industry is still to show a solution set to GSI-191 by the end of this calendar year. Now if you do the time line on that, and they're doing the chemical testing in the summer, it's going to be very challenging for a number of them to perhaps react if they have trouble with their first solution, to come up with a different one, and then test that, so it's going to be tight. But we don't anticipate that this is -- I think you referred to an out there multi-year effort by the industry to do this testing. The testing is to happen in calendar year 2007; and, hopefully, that will allow the industry to provide us the responses to Generic Letter 2004-02 that are due by the end of this year.

And we, NRR, plan to come talk to you in May about the status of the issue then, and we'd be happy to tell you at that point whatever we know about the chemical effects results that are available to us.

MR. WALLIS: Well, I know all that. The thing -- we're taking this high-level view. The thing that's a bit of concern is that all the steps forward in understanding these problems so far seem to have been made by NRC. And, originally, there was no concern with chemical effects, and so there was some concern, and then some work was done, and it was discovered that yes, indeed, there are. And then it was discovered that yes, indeed, they can have huge effects on pressure drop. All this was discovered by the NRC. I'm a little concerned about having the NRC going out of the business of discovering what's going on, leaving it all to industry.

MR. BUTLER: Dr. Wallis?

MR. WALLIS: Yes.

MR. BUTLER: John Butler, NEI. I object to that characterization that it's the NRC who is discovering this. The discovery of the effect of chemical effects came out of the ICET program, which was a industry-NRC jointly sponsored program. Now you could argue - in fact, I would argue that it was industry who put forward that program, and invited NRC to participate in that program.

MR. WALLIS: I thought that industry was telling the ACRS there weren't any chemical effects, before eventually you were persuaded to do this.

MR. BUTLER: That is correct, yes. But, actually, it was ACRS who was putting forward that there were chemical effects, and both NRC staff and industry were saying we did not believe that. But on the ACRS urging, give yourself credit, then investigated whether or not there were truly any chemical effects.

MR. WALLIS: Maybe we should include some other ideas about effects in that case.

MR. BUTLER: But the point I want to argue with you against is the characterization that it's NRC who is always finding these issues, and that industry is always responding. That has not been the case.

MR. WALLIS: Okay. Thank you. Maybe I misrepresented it. History is something we can always investigate if we want to, but we don't have to go there today.

MR. XIAO: This is Tony Xiao again from research. I just want to make it very clear to the committee that we are not going out of business on this research. All we're saying right now is we've planned out the research in the past few years working with the NRR, and in some cases working with industry, like John pointed out. But we are -- all we're saying right now is at this point we believe we have done enough research to be able to really understand the issues, and identify some issues, to let the industry, given all the technical knowledge we've gained from the past few years, to move forward, come up with their specific solutions, if necessary. And we are going to continue to monitor through NRR the industry resolutions. And like Mike said earlier, whenever there's need, we'll jump back in and do the research. All we're saying, at this point we have no plans to do any more, but we're not going out of business.

CHAIRMAN BANERJEE: Let me just summarize what I think, just to summarize my understanding. What you're looking for from the ACRS then is to write a letter saying that at the moment, we feel that the research done has been adequate for its purposes, has identified some issues, and now this has been handed over to NRC, I mean, to the industry to deal with. And then --

MR. WALLIS: I think we've written that letter already.

CHAIRMAN BANERJEE: Yes, so I'm wondering what is it that you really want from us? Do you want us to say we don't -- we see it's fine that you stop research until the industry does something, and then come back, and then you'll do whatever is needed to validate or verify, or whatever? What is it that you want?

MR. GEIGER: Okay. Well, let me say to the point - at this point, we feel that the research, the need for the research identified to-date has been completed. And I know Dr. Wallis agrees with this position, but right now, we have sufficient information on all the sump pool chemistry issues to at least realize that there is a significant problem at some plants, and they may be making modifications to mitigate those conditions. And I was going to present some of the issues in this presentation about what some of these are, but at this point, I thought the - like you said, I would hope that the subcommittee would conclude that, for the time being, what we have done, including the PNNL work, is adequate to sort of wrap-up this phase of the research. And in the event that other issues arise to this chemical testing, it would be handled on an as issues arise basis.

CHAIRMAN BANERJEE: So the real thing that you're -- the real questions you're putting to us is, are you -- is the ACRS in agreement that research should be stopped? I mean, that's what it amounts to.

MR. WALLIS: You've already wrote a letter that said research should not be stopped. You wrote this letter. We reviewed all these programs already, and we looked at the major effects discovered, and we said that we were not sure that it was a good idea to stop research, because there were still -- these questions were not resolved to the point where you could quantify many of the effects, and so on. And I don't see what we could add to that, unless you add something substantial today that we didn't know when we wrote the previous letter. I don't know why we should write another letter.

MR. TRAGONING: Let me jump in here a little bit. Rob Tragoning from Research. Let me relive a little bit of the history and try to get at why we're here today, and try to address your comment, and Dr. Banerjee's comment, as well.

The last time we were in front of you, I believe was June, and then prior to that was maybe last February or so. And during each of those times, we've been presenting the status of the research to-date. And at the time, in both February and June, it was ongoing. Okay? What the purpose of today is, is we've had activities in June that have subsequently finished, and we had activities in June that we hadn't -- that were preliminary that we didn't provide results, so what Erv and the rest of the presentations today are structured to do is to present information that either indicates programs that we haven't presented to you in the past; for instance, the peer review report, okay? That information we have not presented to ACRS in the past, because in June it was still in development and preparation. It wasn't mature enough to present to the committee.

There are several other areas that were not quite finished in June; for instance, the development of the head loss correlation. That work has been completed, as well as some of the - I think the PNNL testing was done. In fact, that's not being presented today, is it?

MR. GEIGER: Just some follow-on testing that we did in response to the initial findings.

MR. TRAGONING: So all today will do will fill in the gaps, and provide additional information for other programs that we haven't previously presented. Now if you choose, based on that information, to go back and revisit any of the positions that you've stated in previous letters, then that would be particularly appropriate. If you choose not to, I think that might be particularly appropriate, as well. If you learn something, or we say something from the peer review comments, for instance, that triggers some additional thoughts that you'd like to capture, that would be appropriate. But other than that, I don't know that we're specifically asking for a letter, as much as we're providing status, and continual update of the programs that we've previously presented in the past. I know there's been a bit of a confusion, but hopefully that will -- hopefully we can come to some agreement.

MR. WALLIS: Met with the commission, I

forget exactly what the date was.

MR. TRAGONING: July, maybe, or August.

MR. WALLIS: After we had met with you in June, and I remember saying that we had our say, that the staff knew what to do. And really, it was something they had to work out between NRR and industry, and we would sort of follow events, but we were not - we had our say. We're not sure we could add anything. Essentially, that's what I was saying at that time, and I think I feel the same today. I mean, this is deja vu, what I've seen in the reports is simply a collection of what we already knew. And the peer review simply confirms, I think what we already thought.

MR. TRAGONING: Right. There are some interesting ideas in the peer review that may need some discussion, and possible consideration. But again, I look at this meeting as simply - as filling out and providing an update on the research that we've done, as well as giving you an opportunity. We've essentially dumped a number of these reports on the committee, and we wanted to use this vehicle to address any comments or questions that you may have had as a result of reviewing those reports.

MR. WALLIS: I'm sorry to get in this

discussion. I read this stuff, and I'd like to say that the Pacific Northwest report - now the most interesting thing they discovered was that the order in which you put the stuff in can make an enormous difference to the pressure drop. Having discovered this in one series of tests, they were told to stop their work. And then Argonne discovered that yes, you could get these enormous pressure drops with certain chemicals. Now you think the rational thing to do is to say ah-hah, those are important effects. Now I want it done right, so I understand what's going on. Instead of that, the attitude of RES is stop the work. We said this before.

CHAIRMAN BANERJEE: And one thing pointed out in the peer reviews is that at least some of the peer reviewers felt there were a number of issues which warranted more work. And, in fact, there was a suggestion that the work be on a smaller scale. They felt these very large experiments were very expensive, and didn't add - perhaps in the beginning they were useful, but to investigate some of these effects at relatively smaller scale, less expensive program looking at some of the issues raised by them would be useful. In particular, some of the peer reviewers raised the issue that development of simulation tool which was based on existing tools were, perhaps, not the right direction to go. There were different suggestions made. And, in fact, some sort of confirmatory or predictive tool which would actually look at some of the non-equilibrium thermal dynamics was suggested, as this was not an equilibrium process. I hope those issues are going to be addressed. You're not just going to talk about the peer review, but actually tell us what you're going to do about the issues we raised.

MR. GEIGER: All I can say to that issue is that it was a very good comment, and we looked at four different --

CHAIRMAN BANERJEE: None of them really --

MR. GEIGER: And none of them really gave us anything.

CHAIRMAN BANERJEE: -- did the job.

MR. GEIGER: So then my question to you is, is what would it take to actually develop those programs. I mean, these industry programs out there have been out there for a long time, so what would it take to complete those to where we could use them? And when you're looking at the time spent, and when we have to resolve this issue, by the time those models or programs proposed would be completed to the satisfaction where they would be useful, it would be too late for this effort, so maybe we could use them for some other effort at some other time, but that's really not the objective of this project.

CHAIRMAN BANERJEE: That was not the feeling that -

MR. GEIGER: So what we decided is we're not going to use --

CHAIRMAN BANERJEE: That was not the feeling that some of the people in the peer review panel had. I mean, they felt that those tools were not appropriate that was used, but that there were appropriate tools. And I can go through all the comments, I read them in detail.

MR. TRAGONING: But I don't know that we had consensus on that issue. We had a number of different views. We had some views that essentially -- I think it was universally realized, and we agree that the method, or the tools that we were using weren't particularly appropriate. However, in terms of what the path forward was, I would argue that there was no consensus. We had five different peer reviewers, and we had comments raising from the development of codes may not be a very useful exercise for this instance. We had some comments that said we could use thermal dynamic codes and, essentially, calibrate our the kinetics to use existing codes. And then we had some comments that said no, you have to fully develop a kinetic code to really address these issues, so I don't know that we had any consensus from the peer reviewers on path forward with that particular issue.

CHAIRMAN BANERJEE: What would be useful, I think, is when you deal with the peer reviews, to summarize their comments, and to tell us what the NRC thinks about it. I mean, you may or may not agree with the peer reviewers, and some of them have written reports which are almost as long as your reports.

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: So it would be nice to hear the main issues that there is, and what the NRC wants to do with it.

MR. GEIGER: Unfortunately, I'm not really qualified - I'm not a chemist, or at least I'm not qualified to respond to all of those peer review issues at this point, and I guess we could --

MR. WALLIS: Well, you could skip all the lists of all the reports, and go to your slide where you talk about what the results are, and what you learned from them. And we should stop interrupting you, perhaps.

MR. GEIGER: No. I understand.

CHAIRMAN BANERJEE: It was useful to know what we are being asked to do.

MR. GEIGER: Well, I guess what I was trying to point out, though, is that if you look, we've done - we have six NUREG --

MR. WALLIS: You've produced a lot of paper. All right. Now what useful results came out of it?

MR. GEIGER: What useful?

MR. WALLIS: You produced a lot of paper.

MR. GEIGER: Paper, yes.

MR. WALLIS: Now what useful results came out of the work?

MR. GEIGER: Well, NIS --

MR. XIAO: We said earlier -- you also mentioned, Dr. Wallis, and that, first of all, we have confirmed some of the, for example, chemical effects. That's brought up by ACRS and we have confirmed that, and industry also right now is taking action on that. And there a lot of useful results came out of all this research. I would say that there are other head loss correlation, that's also a new one. You look at the

6224 correlation several years ago, we realize there are deficiencies in there, so Krotiuk in his contract started to work on that.

MR. WALLIS: Are they going to stand up and say that we discovered that there was an effect with aluminum oxhydroxides. We did a computer research program. We understand the chemistry, we know how to predict when these things happen. We know how to predict what the effects are, we know that there's a critical level of concentration before there's any effect on head, we can predict that for any plant. You're not going to give that kind of a nice crisp presentation.

MR. XIAO: No, we're not.

MR. WALLIS: That's what I'd like to see.

MR. XIAO: That's a very --

MR. WALLIS: Are we ever going to get it?

MR. XIAO: That's correct, I don't think we're ever going to get there, to be able to tell you categorically, or anybody say hey, we've got developed this tool, we understand completely. Nobody has to do tests, or I can tell you exactly how to solve the problem. I don't think we'll ever get there.

MR. WALLIS: Right.

MR. KRESS: The other way it could go is

rely on the plant-specific tests for a relatively clean resolution of the issue on a temporary basis, based on your insights, and then to do confirmatory research to develop this integrated overall predictive model, which I would like to see, also. And use that to say did we do it right? And that could be the --

MR. WALLIS: Otherwise, you may be revisiting this problem forever.

MR. KRESS: Yes. That could be the comment we'd like to make.

MR. WALLIS: Well, we made that comment before.

MR. KRESS: Yes, we've done that before. That's not original with me.

MR. WALLIS: Okay.

MR. GEIGER: Okay. I'm sorry. Is there where you want me to go?

CHAIRMAN BANERJEE: Do you have any information you could give us about what industry is actually doing? Because if they're doing a number of ad hoc tests, they may or may not be addressing the issues. And we have no idea what they're doing in response to this. For example, the peer reviewers bring up issues related to temperature changes that you can basically set up a mass transfer loop, where you're redissolving and depositing stuff. Now there are a whole lot of things which they discuss which weren't addressed up to now, and could be addressed, they think, in relatively small-scale tests. So what is industry doing? I mean, these are reviews that the peer reviewers are making now. If you give the industry work sort of equivalent peer review, the issues would still be there.

MR. KLEIN: This is Paul Klein from NRR. I think in the May meeting we'll try to provide you a good summary of industry tests, what's going on, what results they're seeing, and NRR's perspective on those test results. From my viewpoint, the research has been very helpful, because as we go out and we try to understand the plant-specific testing that's underway, trying to understand the effects of some of these parameters, and to see the effects of different buffers that have shown to be different, and the research results have been very valuable to us, and we have provided feedback to different industry vendors on their test techniques, and probably asked better questions about how they're conducting their tests as a result of the information we've learned from research.

CHAIRMAN BANERJEE: Well, let's go on.

MR. GEIGER: Okay. So as part -- well, this is a summary of what we've learned. So we learned that we can form a gelatinous material in certain post LOCA environments. We've learned that Nukon with sodium hydroxide buffer environments produce a white precipitate that deposits and causes head loss. We've also learned that Nukon with TSP buffer produced a precipitate which deposited on insulation fibers, which meant that now people with TSP and Nukon are going back to see how they can resolve this interaction. Nukon cal sil with a TSP buffer produced a white precipitate, again, which coated the inside of all the piping and so on, which could lead to the mass transfer, as Dr. Banerjee has just mentioned. That is actually a downstream effect that research has not really been involved in, that's, I guess, the --

MR. WALLIS: Presume that these precipitates were all different, because they had different chemicals involved?

MR. GEIGER: Yes.

MR. WALLIS: What were the precipitates then? They were all white, but what were they?

MR. GEIGER: Well, we had calcium phosphates.

MR. WALLIS: Were they actually analyzed? I mean, you don't say.

MR. GEIGER: All of these were covered in previous --

MR. WALLIS: But if you'd say what the precipitate was, that might help us.

CHAIRMAN BANERJEE: Some of these have reverse solubility effects.

MR. WALLIS: Right. You heat them up, they -- right.

CHAIRMAN BANERJEE: Heat them up.

MR. TRAGONING: Yes, we provided all this information.

MR. GEIGER: This has all been --

MR. WALLIS: You don't put it on the screen. If you just say white precipitate, that's sort of vague.

MR. TRAGONING: I agree.

MR. GEIGER: I'm sorry. I was trying to limit to a one-hour --

MR. WALLIS: Would it produce phosphates of calcium?

MR. TRAGONING: Yes, calcium phosphates in the TSP.

MR. WALLIS: Which has a reverse

solubility, doesn't it? You heat it up, and it precipitates, or is that --

CHAIRMAN BANERJEE: Some of them.

MR. WALLIS: Some of the calcium salts do, so you put them through the core, they're going to precipitate in the core. Is that right?

MR. TRAGONING: Well, that's not a specific effect that we had looked at in our research.

MR. WALLIS: Yes, but this is raised by many of the peer reviewers.

MR. TRAGONING: No, I understand.

MR. GEIGER: That's an issue that's being evaluated by the PWR Owner's Group, and their evaluation of effects within the core is that issue of calcium precipitation.

MR. WALLIS: So it's all someone else's work, someone else's responsibility to figure it out?

MR. GEIGER: No, I don't think that's an accurate characterization. I think that industry has a role in this, and NRC does, as well.

MR. WALLIS: All right.

MR. GEIGER: So what we have done, the effect of this research was to prompt industry to investigate alternatives to resolve some of these head loss issues, and there are plants that are looking at changing their buffers. Some plants are removing or relocating problematic materials, like aluminum items. They're installing debris interceptors to keep insulation fibers and things from going towards the sump. They're reducing latent debris, installing jet shields to minimize the amount, or to reduce the zone of influence so that they don't have as much debris generated which could then react in the sump pool.

MR. WALLIS: Do you have shields?

MR. GEIGER: Yes.

MR. WALLIS: You know where the LOCA is going to be, so you put a shield there?

MR. GEIGER: Well, you know where the LOCA is going to be, yes. It's on the RCS pipe, typically, so you would either put deflectors or something like that to protect. And it's commonly done, if you don't want insulation to come off, you'll put a structure in front of it.

MR. WALLIS: Between the pipe and the insulation.

MR. GEIGER: Yes. But these are all things that utilities are looking at to mitigate some of these issues.

CHAIRMAN BANERJEE: One of the issues the peer reviewers raised was the organic materials that were there in the insulation, and their reactions. Right?

MR. GEIGER: Yes. Are you talking about the binders in the insulation?

CHAIRMAN BANERJEE: Yes, yes, yes.

MR. GEIGER: Well, the organics in the fiberglass insulation, the part that's close to the surface will burn-off. Right? And as the temperature decreases across the gradient of the insulation, the outer layers may have some organics. But my experience has been with plants that operate for a long time those temperatures also eventually volatilize a lot of those, so I don't know how much of those have been --

CHAIRMAN BANERJEE: But we haven't seen any assessment.

MR. GEIGER: No, we have not. No.

CHAIRMAN BANERJEE: But in the --

MR. GEIGER: What we have done is in the tests we've used some of these Nukon blankets with this material on it, so in that respect it has been simulated. Are you going to add something, Rob?

MR. TRAGONING: Yes, we did have those organic components within the ICET.

MR. GEIGER: Now there are other issues we looked at, like how about lubricants, like the reactor coolant pump waste oil tank, and that sort of thing, that may add some issues. Now we haven't discounted that that would happen, but typically, there isn't that much, other than the reactor coolant pumps, there aren't any other lubricated - they're usually lifetime lubricated bearings and that sort of thing in the rotating equipment, so you wouldn't expect a lot of oils and things to be in containment, especially since they're a source of fire, so fire protection practices keep plants from putting any lubricants and things in containment.

Now it was brought up, like I said, that the reactor coolant pump waste oil tank would spill water into the pool, but it typically has a gooseneck vent on it so that it would prevent the oil from coming out. There's a trap, and they would also have to be submerged in a pump. Now I agree that we've sort of looked at that and said well, really, that may not be a real problem, but we haven't really investigated in any real detail.

Other carbon parts, I don't know. I guess they're in the LOCA, you could have things come off the insulated cables and things, that sort of thing, but we did not look at that. Well, I'm not sure the

ICET didn't.

CHAIRMAN BANERJEE: Well, this at least be put to bed in the sense that an issue will address that, or --

MR. GEIGER: What's missing, and I agree with you --

CHAIRMAN BANERJEE: Assessment.

MR. GEIGER: What's missing is a real answer-by-answer. And we've sort of answered them in general, and some of the things, as the research was going on. Part of the problem is that the peer review was going concurrent with the research, so when the research was at a certain point, it might not have been possible to incorporate a lot of these things into existing research. Now some of the other things they recommended, we may look at. You're right, we probably should come up with a point-by-point response. We have not done that.

MR. KLEIN: Erv, if I could add to that - Paul Klein from NRR. In some of the industry tests to support their chemical model that are in the WCAP that was submitted to NRC, they evaluated fiberglass that both had binder and did not have binder, and they did not see a significant effect between the two different types of material, as far as what was released into the test solution.

CHAIRMAN BANERJEE: Well, in a more general way, the peer review, which we are going to hear about was, I think, a very useful exercise, and NRC should be complimented on subjecting themselves to this. And some of these peer reviewers are pretty expert at what they did. Now they made some comments, and I think we should tabulate them, would be my suggestion, and say what we are doing about it, if anything. We can say industry is doing this, or this is not important, we don't feel this is - but at least we should somehow - the value of this peer review process is very high, I think, and if you don't respond to something, I think you should say we choose not to respond to it. Okay? That's fine.

MR. GEIGER: I agree with you, and unfortunately, we have not done that on a point-by-point, like I said, as we went along. But historically, since I can't speak to all of the -- how they were incorporated in different research projects, because I was not involved at that time, and my intent for bringing this issue up in this meeting was I figured that somehow a lot of those comments needed to be addressed. And, quite frankly, when I got into it, I realized I was overwhelmed by the number of comments and how it went.

CHAIRMAN BANERJEE: But you could maybe categorize them and say okay, we have evidence that organics are not a problem. This is what industry has already done. They have shown that it's not, we had them in the ICE test, or wherever, so that we've dealt with these in a systematic way. And, actually, they have some comments which you say could lead to a need for additional work, which we are not prepared to do right now. I mean, may as well be straightforward about, and say well, we just don't have the money or something.

MR. WALLIS: Well, another thing is take-up of CO2. Several reviewers talked about take-up of CO2 from the containment --

CHAIRMAN BANERJEE: And the aging of concrete.

MR. WALLIS: Some carbonizing things, which hasn't been part of the discussion so far at all of chemical effects. The thing that surprised me a bit was there were some really interesting comments by reviewers about things being misleading, or erroneous, or totally inadequate and so on, and so on, and then in the introduction someone presumably from NRC said that the principal objectives have been met, and the reviewers confirm the technical adequacy of the research programs. It's hard to see from some of the comments of reviewers that that was true.

CHAIRMAN BANERJEE: Maybe the overall gist of it may be that way, but --

MR. WALLIS: As long as it was very preliminary, as long as this was just try it and see what happens, I think you could say that you did meet your objections, but then there was no follow-up. It was all left to industry to follow-up.

CHAIRMAN BANERJEE: Well, even if we leave it to industry, it would be valuable to convey to them the issues that came up in the peer review, and what the disposition of these issues was.

MR. WALLIS: How about CO2 take-up? Are we now going to require that industry do that?

MR. GEIGER: Well, when you model the sump screens and you do these flow tests, it's open to atmosphere because they're done at atmospheric conditions in these test facilities, so they would -- unless you put a spray to it that would increase the CO2, it would replicate pretty much what you see in containment for the duration of the accident, except for the first initial spray. Now how much CO2 is in the volume of the containment, that would affect what happens. I guess I can't really make a judgment on that. I would think that there's negligible CO_2, looking at what the percentage of CO_2 would be.

MR. WALLIS: Well, someone actually did the quantitative analysis.

MR. TRAGONING: Rob Tragoning, and let me comment because Professor Banerjee and Professor Wallis have both raised some good ideas for path forward here. And the other piece of information that's missing, as part of this peer review, while we had all the experts together, we also conducted a PRT. Now we're not going to -- we haven't finalized or we haven't summarized the results of the PRT. We're not going to summarize that today; although, many of the peer review comments you might imagine came back up again in the PRT. But the idea behind the PRT, we got - because of the volume of peer review comments that we got, we wanted a way to try to prioritize and rank, and get some idea, at least from the peer reviewers' perspective, which comments were, they felt, most important or appropriate for future consideration. So one of the exercises that we're doing is we're continuing to evaluate the PRT responses, and evaluate which of those, again, issues the peer reviewers felt were most important. And my expectation would be that an outcome of that would be that the PRT results would be shared with both the industry and NRR, and there would be some consensus, or some joint resolution that we would try to address those and figure out which ones of these we feel are issues, and which ones we feel like we've appropriately addressed. So that's the other piece of missing information that we're not going to have today, but I think it ties into some of the path forward issues or direction that you've been discussing.

CHAIRMAN BANERJEE: Okay. That's good.

MR. GEIGER: Now depending on what actions the plants take, a lot of these may be moot, a lot of these suggestions. It depends on how much fiber we end up with in which plants, so it's going to be very plant-specific. And I agree, at the point when certain plants make certain decisions, we need to have the capability to evaluate what they're doing. And, perhaps, that's what we need to look at. But I also take your suggestion on actually tabulating all of these peer review comments and coming out with some sort of either resolution and why we did it, why we didn't do it, or what needs to be done. That's a point well taken.

CHAIRMAN BANERJEE: It would also be

useful if all of you took an impartial look at it and said that well, should we really - in spite of what management says, should we really have, as some of the peer reviewers suggested, a relatively small scale program in place to look at some of these effects? So I think it would be nice to divorce this a little bit from what management wants, and what is actually required. They're two different things.

MR. GEIGER: Okay.

MR. TRAGONING: Again, that's not trivial. Let's go back over the history. We started the chemical experience with conducting a very large number of small scale tests, and the immediate feedback we got from that is they were too simplistic, and we need to conduct multi-scale integrated testing so that we could look at the synergism of all these various variables. So now we're at that point where we did the multi-scale, larger scale testing, and we did find out some things, some combinations that were particularly appropriate.

Now I think what the peer reviewers were recommending to say okay, at that point, now you potentially need to go back and do some additional smaller scale tests, focusing on the effects that were identified in the integrated test to try to get a better understanding of what the effect is of key variables on the specific precipitates. And if I look at some of the work we've done, as well as some of industry's work, there's certainly been a move back to more small scale experiments. Now it's not clear to me yet what additional small scale experiments may be needed, as well as what additional integrated tests may be needed. And I think that's where the industry's research that they're doing now to identify what issues really remain once the specific mitigation procedures were developed, I think it's just particularly appropriate and important to determine which way we really need to go at this point.

CHAIRMAN BANERJEE: Okay.

MR. XIAO: This is Tony Xiao from Research. Let me just add to what Rob said. Consistent with what I said earlier, we're not going out of business. What we will do is to work with NRR very closely. We will watch, we will monitor what industry's implementation plans are in resolution of these issues. And based on that, we'll make a decision down the road as to what - any research would like to do small scale.

MR. GEIGER: And as Rob mentioned, we started out with a small scale, and then this is the issue, and I've always faced this quite often, is that you do the small scale - well, how do you know it's really representative of what the macro is doing, so then you do macro tests to actually simulate - and I've worked at the utilities where we've done tests, put in the parameters of our plant and we test it, and this is what we have. There's no budget assigned to do esoteric research and things, we try to solve the engineering problem at-hand. So, I guess, that's why I'm so used to, but when somebody does a small scale test, how does it look on a macro? And if you do macro, then well, how about if we do calculations to calculate it. You do calculations, well, you have to demonstrate it with some tests, so it's a circle. You do your test, then your calculations, so on, so it ends up that you have to do all three. And I think we've pretty much done all three of those things. We've done the small scale, and I'll go later on into some additional testing that we did at ANL, the next presentation. We did some testing at ANL at the request of NRR to investigate the surrogate recommended by the WCAP, and also then we looked at some --

CHAIRMAN BANERJEE: What is the surrogate?

MR. GEIGER: The aluminum oxyhydride, the way they produce it.

MR. XIAO: That's scheduled. We'll start that at 10:50 this morning.

CHAIRMAN BANERJEE: Okay.

MR. GEIGER: Okay. And that was done by Dr. Shack. Unfortunately, he's not here. Since the budgets for the research had sort of lapsed and the contract's run, I didn't feel I could impose on him to make a presentation, so I'm presenting his paper today. I'm sorry. But we scheduled for today. And I'm sorry if I misread the subcommittee's intent on what you wanted to see today, but when I present some follow-on studies and chemical effects head loss research done at ANL, and then -- well, the peer review, which, after discussing with you, I think we've already discussed the peer review, and we know where it's going. This afternoon we'll have a presentation on the pressure drop research done, and then the correlation down at --

CHAIRMAN BANERJEE: But these two, will they take into account chemical effects, or they're just the --

MR. GEIGER: These are done with - and this is the one where Dr. Wallis had said that the - we found out that the sequence of addition of the things - but there are no chemicals. These were done using fiberglass calcium silicate, and then we did some coating chips to see what coating chips would affect. It was basically a particulate --

CHAIRMAN BANERJEE: And these -- I remember Mr. Krotiuk's previous presentations. Refresh my mind as to whether it takes the sequence of deposition into account, or not?

MR. GEIGER: Yes, it does.

CHAIRMAN BANERJEE: It does.

MR. GEIGER: It was a topic from the last meeting, I understood the subcommittee was interested in.

MR. WALLIS: So it would appear that you're asking ACRS to give you feedback on these four items that you're going to present?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: And what will you present to the main committee next week? Right?

MR. GEIGER: Well, I'm not sure how the main committee meeting was scheduled. I'm not sure what I'm going to present to the main committee at this point. I don't know if anybody has any suggestions.

MR. XIAO: At this point - this is Tony

Xiao from Research. I assume we'll present the same material, unless you have other ideas, or the main committee wants to do something. It's the same material.

MR. WALLIS: Until we see it, we won't really know.

MR. KRESS: Normally, we try to give some guidance on what we want to hear.

MR. WALLIS: Well, I guess you've got to do better than you did in the first hour today.

MR. XIAO: We certainly hope so.

CHAIRMAN BANERJEE: All right.

MR. WALLIS: Okay. So are we going to the South Sea somewhere here?

MR. GEIGER: Yes, that would be good, but a day like today with the weather out there. Is Paulette here?

MR. TRAGONING: She's getting her memory stick. Do you have your presentation?

MR. GEIGER: It's on here, unless she changed something.

CHAIRMAN BANERJEE: Well, actually --

MEMBER WALLIS: Are there slides?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: How did you select the peer reviewers? It was a very --

MR. GEIGER: Well, maybe Rob could respond to that.

CHAIRMAN BANERJEE: In fact, you made a comment that you got suggestions from ACRS and staff.

MR. GEIGER: We looked at -- well, experts in various fields that were related to the power, like radiation.

CHAIRMAN BANERJEE: The point that was weak in the peer review was the pressure loss parts. The chemistry was strong, but we didn't get enough feedback on the pressure loss.

(Off the record comments.)

MR. CARUSO: I have one more piece of information. Mr. Scott from the staff told me this morning that the staff had sent out a letter regarding the Research use of the information from -- the use of this research information, and although I'm on the CC, I don't recall receiving it, and I'll pass it out right now. So we're still waiting for --

CHAIRMAN BANERJEE: Should we switch around the two presentations? Someone said she went to get her memory stick, but I have it here.

MR. WALLIS: Are we on the record with all this discussion here?

MR. CARUSO: Why don't we take a little --

CHAIRMAN BANERJEE: Yes. If she's not here, then --

MR. CARUSO: Why don't we take a little break here.

CHAIRMAN BANERJEE: Okay. Let's go off the record, take a small break.

(Whereupon, the proceedings went off the record at 9:38:38 a.m., and went back on the record at 9:45:44 a.m.)

CHAIRMAN BANERJEE: Okay. Let's go back into session. Are you ready, Paulette? We're back in session.

MS. TORRES: Good morning. My name is Paulette Torres, and I represent Office of Research, Division of Civil Engineering and Radiological Research. I am the --

CHAIRMAN BANERJEE: Bring that a little closer.

MR. WALLIS: Is the mic on?

MS. TORRES: Peer review of GSI-191 Chemical Effects Research Program. The purpose of this presentation is to primarily present the reviewers' significant key findings. NUREG-1861 described the Chemical Effects Peer Review assessment process, and summarized the reviewers' significant findings. NUREG-1861 also compiles the formal review reports received from each peer reviewer. The final assessment reports of the peer reviewers are included as appendices to NUREG-1861.

The Chemical Effects Peer Review consisted of five members, and it is important to mention that this review is not a consensus review. Each reviewer was asked to provide an individual evaluation based on their particular area of expertise, and the Peer Review NUREG-1861 was published December 2006.

The objectives of the review are to assess the technical adequacy of research activities related to the chemical effects in pressurized water reactor sump pool environments, to have the peer reviewers recommend improvements to research programs in the area of chemical effects, and to identify additional important technical issues that should be addressed. And to attempt to gain a thorough understanding of the relevancy of these chemical effects in the post LOCA environment.

The research project addressed by the peer review included the integrated chemical effect testing at Los Alamos National Laboratory, the ICET follow-up testing and analysis, also known as the aluminum chemistry research, also conducted at Los Alamos. The chemical speciation prediction, also known as the Thermal Dynamic Simulation, conducted at the Center of Nuclear Waste Regulatory Analysis, and the chemical effect head loss research conducted at Argonne National Lab.

This table, you have seen it before, but we wanted to, again, present that the chemical effect consisted of five members selected from industry and academia, and they were selected for their diversity and their affiliations, and technical expertise. The review group participated in kick-off meetings and final meetings, which promoted the discussion, and enabled the members to exchange information, and address questions.

The members developed individual preliminary reports, as well as individual final reports, and those were provided as appendices to the NUREG, that provided the formal assessment of both the prior and ongoing research activities. Next slide.

I just want to say that the summary of key findings - they are part of the NUREG, and I just want to highlight them, because there were a lot. I'm just going to highlight the ones that we thought were very important to share.

MR. WALLIS: It's strange. I mean, bullet one and bullet three seem to be in disagreement.

MS. TORRES: For bullet one --

MR. WALLIS: You've simulated the principal variables, and yet the ones which you haven't simulated maybe have the most impact. It doesn't seem to make any sense.

MR. GEIGER: Those were the findings, so I guess -- I mean, this -- in other words, what this states is there are different reviewers, there are five reviewers and different reviews --

MR. WALLIS: Okay. So that's two different people speaking. Right?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: I guess, again we come back to how you might want to present this, that when you have something like that on a slide it really begs the question. I mean, if you put it as individual reviewer comments, and Reviewer 5 said this, and Reviewer 2 said that, that would be --

MR. GEIGER: Well, it wasn't a consensus so there wasn't a vote, but yes. Okay.

MR. WALLIS: Number two is the vaguest possible conclusion.

CHAIRMAN BANERJEE: What you are showing is the texture of the reviews here. All right.

MS. TORRES: Well, to support number one, bullet one, I got that they agree on the type of materials that were presented in the research testing, and they said that they have been appropriately selected.

MR. WALLIS: Well, my summary of the key findings is that they all agree there's a long way to go before you have much understanding of what happens chemically in a sump.

CHAIRMAN BANERJEE: Wouldn't that be better to say here? I mean, that --

MR. GEIGER: Would we be done then?

MR. WALLIS: That's really what they say. They talk about all kinds of species which could happen, and all kinds of other effects which could occur, and so on. There's a long way to go before you really have confidence and you know what's happening.

CHAIRMAN BANERJEE: And then you could say why you don't agree with that. That's fine.

MR. GEIGER: Well, it's interesting, because if you look at this comment, the third bullet --

MR. WALLIS: Which says - now, I'm

reading, I'm quoting from the report here.

CHAIRMAN BANERJEE: Yes.

MR. WALLIS: I think this is the summary written by you guys. The reviewers agree that the methods used within the ICET program were not sufficient to characterize and analyze chemical byproducts. That's a statement in the peer review report, and I think it's in the summary, so it must come from you folks. That doesn't seem to agree with what you put up here.

MR. KRESS: That's probably the source of the fourth bullet.

MR. WALLIS: But the top one says, "inadequately simulated", and here it says, "the methods used were not sufficient."

MR. KRESS: Contribute that first bullet to one reviewer.

MR. WALLIS: One reviewer. Okay. All right. I'll -- maybe we should move on then.

MR. KLEIN: Paul Klein from NRR. I think the first bullet also might address things like the ICET tank represented containment materials, the boron level approximated the equilibrium temperature of the pool, so I believe it's related more to those parts of the test.

MS. TORRES: For bullet two, the reviewers suggest a comprehensive evaluation of the physical and chemical properties of the observed precipitates, along with a detailed evaluation of all the data to better understand the product formation. And they agree that the temperature has a significant effect on solubility and the types of compounds that will form, and also recognize that temperature is a difficult aspect to model, and they recommend further work.

MR. KRESS: Well, in that fourth bullet, they seem to be concerned about particles and particle sizes. But I've been under the impression that these chemical effects are due to gels, gel-like substance. I'm not quite sure I understand what they're focusing on there. Could you comment on that?

MR. GEIGER: Well, part of the -- because we looked at also the particles that could affect the insulation debris bed, and the particle size as far as the property of the coagulate and so on that forms. I'm not sure these are particle sizes actually or just in a different molecular structure and size of the molecules.

MR. KRESS: Yes, you might get some of the molecules out of an x-ray, but transmission would have to -- okay.

MR. KLEIN: Paul Klein from NRR. I believe the comment on particle size probably came from Dr. Woo Chin who specializes in filtration, and part of his comments were that the particle size is important for trying to understand the impact to head loss.

MR. WALLIS: Well, I think what you got from that was that - you can't just talk about a precipitant. In some experiments, they got a heavy snow, and in some cases they got very, very, very fine, some cases they got precipitate you couldn't see at all, yet it was clogging up the screen, so it's not good enough to just say we have calcium phosphate precipitate. It's very important what its physical nature is. And this isn't something which I think anyone has really researched in these programs. They just got precipitates, and they've taken whatever they've got, but they haven't said what were the conditions that gave us a coarse one, or a fine one, or an unrated one.

MR. KRESS: Well, if you're going to try to understand the effect on pressure drop, you'll probably need that.

MR. WALLIS: You need that.

MR. KLEIN: I think ANL did a number of

studies trying to evaluate the effects of different parameters on the size of precipitate that formed, and whether it was amorphous, or crystalline. And Mark Lasky at LANL did try to evaluate things like the amount of hydration, so that work has been done.

MR. WALLIS: Yes, there were some observations, there were some observations, but I don't think there was a predictive capability that said in a sump, if you know what your sump is, this is going to be the size of your precipitate.

MR. KLEIN: I agree with that statement.

CHAIRMAN BANERJEE: I think one of the things the reviewers pointed out was that we didn't take into account the effect of the very high radiation fields in --

MR. WALLIS: It's on the next page.

CHAIRMAN BANERJEE: Oh, is it?

MR. WALLIS: It's on the next page, yes.

CHAIRMAN BANERJEE: Okay.

MS. TORRES: For the chemical fixation prediction program, the reviewers agree that this program does not explore the existing capabilities of the selected host to their full advantage. Two physical effects in that model were the radiation field from the fuel, and the layer of corrosion products on the interior surface of the RCS. And reaction rates, also known as kinetic, are not handled well by the modeling software.

MR. WALLIS: What does the NRC think about number two? I mean, has there been an assessment of the effects of radiolysis, or has there been an effect -- assessment of the effect on these ferritic corrosion, or iron basis interacting with what's in the sump? Has there been an assessment of that, or is it just comment that the reviewer made?

CHAIRMAN BANERJEE: Well, this is a comment which is common to a lot of the reviewers.

MR. WALLIS: So is somebody following up on that?

MR. KLEIN: Paul Klein from NRR. Yes, Dr. Wallis, we are. We've asked those questions of industry as part of the RAI that went out with WCAP 1650NP, so we'll be following that up with industry. And, ultimately, we trying to get a contract in place to bring in some more technical expertise to help us on a number of issues, including this one.

MR. KRESS: This has to do with recirculation to the core?

CHAIRMAN BANERJEE: That's one shield, but gamma, high gamma field --

MR. KRESS: Gamma is not going to do it to the sumps.

CHAIRMAN BANERJEE: No, but there --

MR. KRESS: Recirculation to the core.

CHAIRMAN BANERJEE: -- is also a concern about any releases would lead to alphas, which have a short range effect, and they could then have an effect on the sump. I mean, it's a complicated business, and also got to do with any released fission products.

MR. KLEIN: And we'd also have to look at how much shielding is afforded by the sump pool, so what kind of radiation you're looking at.

CHAIRMAN BANERJEE: Yes. I think Digby points out iodine.

MR. KRESS: Yes, but we're -- in this problem, we're only dealing with what's possibly in the gap of a fuel element, and this is not --

CHAIRMAN BANERJEE: Right.

MR. KRESS: Fission products aren't a problem.

CHAIRMAN BANERJEE: Yes. I think you really need to address the issue and put it to bed, if necessary. They're concerned about peroxide as well, right? They're also concerned about hydrogen peroxide forming.

MR. GEIGER: Yes, they mentioned hydrogen peroxide generation, yes, and its reaction.

CHAIRMAN BANERJEE: Well, there's so much -- I mean, we are coming back to the point that I think you really need to address this in a more systematic way than you're doing here. I think you need to tabulate them, take the remarks, see what your response is. If you don't know, just be honest. Say it needs to be further assessed.

MR. GEIGER: The question is how would we publish that, what kind of a document would have that? Maybe another NUREG --

CHAIRMAN BANERJEE: Well, maybe just internally to present to us. If you come in front of the main committee next week, I think it would be useful to summarize what the three or four main comments each reviewer made, and what your response, and what are common, or something like that, what are the issues. And some you can address, some you can't, some you're giving to industry to address, whatever action you're taking on it, that would be useful.

MR. GEIGER: Point well taken. Should we finish this?

MR. WALLIS: Well, there's an interesting peer review by Brian Sheron in the preface, which says that, "This work provides initial understanding and insights." I agree with that, but that sort of raises the question, where do you go from here, if anywhere, if you've just got an initial understanding and insight.

CHAIRMAN BANERJEE: I guess you're coping with the feeling that ACRS had, and we put in our last letter, that some research should continue. And looking at this peer review, the peer reviewers seem to feel the same way. Now if you say that you're going to turn all this over to industry, as you've done, then I think some justification --

MR. GEIGER: Be provided.

CHAIRMAN BANERJEE: Yes, should be provided saying we don't believe that we need to do this, but industry can do it, and we'll monitor it. Something happens, we'll take care of it then. There has to be some specific response to each of these points.

MR. GEIGER: Well, if you look at the reports, there's well over 100 different points going paragraph by paragraph on the different, and we did not address them because, one, a lot of these -- as the research developed, and this peer review was done pretty much after we had some research, so a lot of times it's difficult to go back. And where we could, we went back, because the reviewers also were involved with the labs. So, in the end, we got the product we got.

CHAIRMAN BANERJEE: But now in retrospect, we can look at these and divide them into major comments or minor comments. You can see the minor comments, we don't need to worry about too much. Here are some of the major comments. Some of these were addressed during the research, some of these were not, then what do you intend to do about this? If you think they're important, it's also your judgment call. You guys have been doing the research.

The part of the peer review which I thought was not particularly strong was related to pressure losses compared to the chemical part of it, and the interpretation of the pressure loss experiments, and what to make of it, for what it's worth. Let me give you my feel for it.

MR. GEIGER: Because I guess the experts we had were more in the other fields, and really were not hydraulics-type people, so that didn't catch their interest as much as the chemistry that was occurring in the pool.

CHAIRMAN BANERJEE: All right. So there's one whole set of experiments which were --

MR. GEIGER: Well, because that's --

CHAIRMAN BANERJEE: You asked them to look at three sets of experiments - sorry, one study which was simulation, the other was the ICET tests, and then the pressure loss tests. And the pressure loss didn't get the same --

MR. GEIGER: Review.

CHAIRMAN BANERJEE: -- examination.

MR. GEIGER: I guess, not to be simplistic or something, but I guess originally the GSI-191 started out as effects on ECCS performance. Correct? So we did all this chemical testing and so on, and the particulate testing to see what the products were. And then at the end when we plugged them into some head loss test, we realized we had a problem, so we sort of demonstrated what the issue was. Now I understand that's sort of simplistic, and it's not a holistic research approach, but right now we know that there are some issues with these chemicals going to the sump, so now it's up to the industry to show that either these chemicals or the blast doesn't get to the sump, or whatever. And then if, at that point, there are issues that still are out there that we need to look at, then I think we would look at it. Again, getting back to the thing, even with this peer review and all these recommendations, at this point we sort of felt that --

MR. WALLIS: And these aren't just recommendations, they're statements they are quoting. Shannon says, "There is a high possibility that the sump screen will encounter head loss problems during post LOCA recirculation. It's a statement, quotable statement. And he says, "The total suspended solids were in the range in which rule of thumb expects plugging in a few days, one to three days." This is what he says, right? He makes these statements, and they're in a published NUREG, so I'm not quite sure how the agency handles things like that.

MR. KLEIN: Paul Klein from NRR. I think from our perspective, we understand the issue, but keep in mind the one thing that the peer review panel did not have access to, is industry's response to some of these issues. And you'll find that for a number of plants that have problematic materials, or a bad combination of buffering materials, they're taking action to try and prevent the problem from occurring, rather than show that they can accommodate the head loss, because the test results from both NRC sponsored and industry sponsored testing have shown that it doesn't take a whole lot of hydrated amorphous precipitate to cause problems in head loss space.

There is a number of things that are being done to try and not put yourself in a position to have a continuous fiber bed, and have amorphous hydrated precipitate impinge upon that in a post LOCA situation.

CHAIRMAN BANERJEE: Well, what --

MR. WALLIS: Does this happen very often? Sorry.

CHAIRMAN BANERJEE: Go ahead.

MR. WALLIS: I was just thinking that this is an interesting situation, because so much of these technical problems get resolved between the NRC and industry, and the public, all the experts apart from the ACRS don't sort of get involved in the process. And here you've got an open process where you've solicited peer reviews from people not involved directly in the problem, or in the regulatory framework, and they've written this report. This is sort of an outsider's comment on what's going on. Just reading it like that, it raises all the questions about what's going on. Are we just going to leave it there in that context, or is it going to be food --

MR. GEIGER: Well, I think --

MR. WALLIS: -- or what is it?

MR. ABDEL-KHALIK: I think you heard Rob Tragoning earlier speak about a PRT that was done with the peer review panel, and a ranking of issues was done by the peer review panel themselves. And I believe he commented that he owed, or not he, but research was planning on developing that further, and bringing it forward for review.

MR. GEIGER: Getting back to the specific comment where the statement was made that there's not particulate to create clogging within a couple of days he said. Right? Well, that right there is a conclusion. I'm not exactly sure what you're looking for for research to do with that, or -- I mean, right now that's out in the public --

MR. WALLIS: Well, if I were a lawyer, if I were a contesting party's attorney, I would use these quotes. I'd ask for a response.

MR. GEIGER: From NRC or from?

MR. WALLIS: From the agency.

MR. GEIGER: And I think we can say that we are responding.

MR. WALLIS: You're lucky I'm not a lawyer, or don't intend ever to be one.

MR. GEIGER: Well, right now -- these

reports are all public, and everybody is aware of them, so industry --

MR. WALLIS: Which is good.

MR. GEIGER: We're getting back to the same thing, is that we have -- actually, you have to give the NRC a lot of credit in that they have demonstrated that there are some of these issues that are real. All right? That's where we are. And now we're trying to cope with the real issues. That's where it's all at. And the bottom line is going to be, and what is occurring is that individual plants are doing testing based on their sump designs, their chemicals, their buffers and the insulation, and whatever else they have to come up with a viable sump screen design so that they meet the minimum MPSH requirements. That is basically where the whole thing is right now. And it's in industry's hands to develop that, and that's all we can offer. We can't go and design their sumps for them or anything.

CHAIRMAN BANERJEE: Of course, but on the other hand, some issues raised here go a little bit beyond that, because we were aware of these issues with downstream effects. But, clearly, there is an issue with deposition in the core, solubility, all these things have been brought up by the peer review.

MR. GEIGER: And these are supplemental to the original task or scope of the whole GSI-191, I think, because they were sump, ECCS. Right? Now we're into the core, which right now - because research is not involved in downstream effects. As Paul had mentioned, there are a lot of efforts within NRR to address downstream effects, and I can't really speak to any of those. I just know there's some analysis being done --

CHAIRMAN BANERJEE: Well, I realize you're not involved with temperature gradient effects, but that has been brought up as one of the main - one main issue.

MR. GEIGER: I guess what it is, is we need to address that.

CHAIRMAN BANERJEE: Yes, one way or the other.

MR. GEIGER: We need to address that, yes.

MR. WALLIS: Well, how will you address it? You will require that industry do experiments with heat transfer surfaces and chemical effects?

MR. GEIGER: Well, I guess we need to get together with NRR to see how that would work.

MR. WALLIS: How are you going to simulate the core environment with a very high radiation? Are you going to do that? That's not an easy experiment for industry to do unless they put it through a real core.

MR. GEIGER: I guess one of the things we would have to look at is how much calcium really is there? See, these are issues. My experience with plants is that calcium - if there's very little calcium by insulation that's exposed. I would be surprised if any number have a significant --

MR. WALLIS: Well, there's concrete dust.

MR. GEIGER: And there's concrete dust. And, typically, the concrete and everything is painted to facilitate decontamination. It all has many layers of epoxy on it, so now there are some plants that may have bare concrete. But, see, all of these are very specific items, specific to certain plants. Now if one plant has all this concrete, yes, we need to - I guess we need to look at it, because then they would have a problem. But until it's shown that there's really a lot of bare concrete, I guess it would be something that NRR would need to address with the different utilities, whether there's an issue. I think we need to discuss that within NRR.

MR. TRAGONING: And if I could just add to that a little further, I think the WCAP testing showed that the contribution of total calcium from concrete was relatively small compared to contribution from things like cal sil, or even insulation. And the peer review agreed with the assessment that the contribution from concrete would be relatively small.

MR. WALLIS: I think they also said it's been around a long time. It's going to be carbonate by the time --

MR. GEIGER: That's correct.

MR. CARUSO: Well, it's interesting, we have this letter that Mr. Scott sent out on the use of this information, and for the peer review it says, "The staff will apply insights from the NUREG in evaluating uncertainties. The staff has also used the insights to assess generic industry approaches. However, the overall applicability of the peer review comments to the staff's regulatory implementation in GSI-191 is limited." What does that mean?

MR. GEIGER: Well, that's an NRR letter, but I think what it -- do you want to respond to that?

MR. KLEIN: I'll take a crack at it. What we tried to communicate in that letter was that there was good information within the peer review panel. We've taken some of the issues that were identified by the peer review panel, and we haven't put our heads in the sand. We've asked industry, in some cases, to address some of these issues. I think I mentioned earlier, we're trying to get a contract in place to bring in additional technical expertise on some of these issues to help us out. The last part of your statement that you read, though, was intended --

MR. XIAO: Not my statement.

MR. KLEIN: I'm sorry. The last part of the statement that you read was intended to get the thought across that the peer review panel comments by themselves didn't provide regulatory direction for us, that they identified issues, or important parts of the research that we would consider, but it was not a direct link to, for example, reviewing Generic Letter 20004-02 Supplemental Response.

CHAIRMAN BANERJEE: What is the point of having that last sentence in your regulatory use of peer review? It seems superfluous to me. "However, the overall" - also seems a bit God-like. "However, the overall applicability of the peer review comments to the staff regulatory implementation of GSI-191 is limited." Why add that?

CHAIRMAN BANERJEE: I don't understand what is limited here.

MR. KLEIN: I think the limitation is that

- and you take any of this research, and it might identify important parameters, or key issues, but it does not always provide a direct link to a plant-specific problem, and so that's the limitation that we're trying to identify.

CHAIRMAN BANERJEE: Is that what you mean, that some of these comments do not apply to plant-specific problems?

MR. SCOTT: This is Mike Scott. As we've said a number of times, one of the constraints of the resolution of GSI-191 is the tremendously plant-specific nature of this issue, and every aspect related to it. And so what Paul was trying to get at is that a lot of this research information - and you'll see this theme in that memo in several places - a lot of this information is intended to support the staff in reviewing what the industry turns into us plant-by-plant. And because it's plant-specific, a generic statement about a particular problem may or may not apply to each of these plants. And the method that's been chosen, and we understand you have concerns with it, is to identify the problem to the industry, and review what the industry does in response to it. And then based on what the industry does, or maybe does not do, we might consider additional research. And that's the path that's been taken, and that memo is intended to speak to that.

MR. WALLIS: Well, there's a question - let's take anything, like this business of corrosion products on the vessel, the layer of oxides of iron. A question has been raised by these reviewers. It may well be that industry doesn't do anything about this. What are you going to do? Are you doing to do anything about it, or just leave it out there as a question?

MR. SCOTT: If there are unanswered questions at the end of this process that we're going through now, which will wind up presumably next year, since the responses come in towards the end of this year - if there are unanswered questions, and there undoubtedly will be, then we will have to assess for each of those questions whether they are significant enough that they need to be followed up in some manner. And if they need to be followed up in some manner, what that manner is.

MR. WALLIS: Someone has to go through all the peer review, and look at all the questions raised, and at some point, decide on your response to it.

MR. SCOTT: We have to look at the questions that are out there at the time. I don't know that we have signed on --

MR. WALLIS: I don't know what that means.

MR. SCOTT: Let me finish. I don't know that we have signed on to go through each peer review comment. That's a good suggestion that we'll certainly consider.

MR. WALLIS: When you say a question arises, now whose question, is it your question, the reviewers' question, industry's question?

MR. SCOTT: Ultimately, in the resolution of the generic letter, it's the staff's question.

MR. WALLIS: It's your question, so if you choose not to ask a question that a peer reviewer asks, you've got to have, probably, some justification for that, it would seem.

MR. SCOTT: It would certainly seem reasonable that if a question has been raised by a knowledgeable source that we should have an answer to it, yes.

MR. CARUSO: Now you said also that this peer review points up the fact that the resolution is very plant-specific, yet the staff has told the committee on several occasions that it only plans to sample some of the licensee responses, and does not plan to review each individual plant response. How do you fit those two concepts together?

MR. SCOTT: I don't recall having made that statement, Ralph. What we are going to do, and we are doing, as I've said in past meetings, is we are auditing a sample of plant responses to the generic letter. We are attempting to get two samples from each vendor's solution set; so that is, we'll do two plants that use AECL as a vendor, for example, two plants that use General Electric, and so on. And from that sample, we are providing the information to the industry to say here are the issues that are identifying. I did not say earlier, at least I didn't intend to say, that we're not going to look at the generic letter responses that come in from the individual plants, because we are. The question of the depth of review of the individual plants will largely be based on what we find in the audits that are ongoing.

MR. WALLIS: Well, there is a question not just of what the chemicals do in the sump, but what the chemicals do in the core, isn't there? What they do in the whole part of the -- the whole circulatory system.

MR. SCOTT: Yes.

MR. WALLIS: Is that part of your concern?

Do you know the questions to ask in that regard?

MR. SCOTT: We are working on - I should say working with the industry. They are planning to turn in a topical report on downstream effects, both in vessel and ex-vessel. The ex-vessel report is in-house, the in-vessel report is not yet in-house.

MR. WALLIS: It did include chemical effects, right?

MR. SCOTT: I believe that's correct, yes. That reported is expected to come in-house in May, but it's going to be really tight getting that review to support the December 31st deadline for the generic letter.

MR. WALLIS: Does this material boil when it goes through the core in recirculation phase?

MR. SCOTT: Does what material boil?

MR. WALLIS: The coolant, the recirculated coolant containing all this stuff.

MR. SCOTT: There will be boiling in the core, yes.

MR. WALLIS: It will boil. So, presumably, you need to know something about what happens to it when it boils?

MR. SCOTT: And the concentration that occurs to --

MR. WALLIS: Do you guys know anything about what happens to this soup when it boils?

MR. SCOTT: These questions have been asked of the industry in conjunction with their soon to be submitted topical report.

MR. WALLIS: So my question was, do you folks know anything about what happens when it boils? I wasn't asking what industry might know.

MR. TRAGONING: I think we do know what happens with some of these materials as they're heated. I don't believe we've run a specific test, or we've taken it to boiling and continued to boil it for a long period of time.

MR. WALLIS: You're very dependent then on what industry tells you.

MR. TRAGONING: Our currently planned resolution process depends on the industry to resolve the issues that have been identified to them, yes. If that approach does not result in success at the end of the day, then we'll come up with a different one, which might, as we've said several times, might involve additional research.

MR. WALLIS: Is there any historical precedent of a major technical issue that was resolved solely by industry?

MR. TRAGONING: I believe you've asked that question before, Dr. Wallis, and --

MR. WALLIS: Well, we're still thinking about that. I mean, in the old days it seemed to me that almost all the key research was done by NRC, but that was when there was all the money for it.

MR. TRAGONING: I'm afraid I'm not going to be able to answer that one.

CHAIRMAN BANERJEE: Shall we move on then?

MR. WALLIS: Yes, we should move on.

MR. GEIGER: Where are we? I guess my whole presentation from here on changes a little bit.

MR. KLEIN: Before you leave this slide, could you give us specific examples about some key findings or recommendations that were, in fact, incorporated into the research?

MR. GEIGER: This statement is based on conversations I had with people who previously had some of these projects. I don't have any specific --

MR. WALLIS: I haven't found any examples. That's a good question.

MR. XIAO: I think in some of the chemical speciation modeling, they did additional work that reflected the reviewers' comments, and you'll see that in some of the peer review panel comments. I know one of the things that was evaluated at ANL was use of sodium aluminate instead of aluminum nitrate to produce the precipitate based on a peer review panel comment, so there were a few things that were done.

MR. GEIGER: Also, I'm sorry, I do not add on a CMWRA, thermal dynamic simulation. Some comments were taken into consideration, additional analyses - so that much I know.

CHAIRMAN BANERJEE: When did the peer review finish, in June last year, actual work of the --

MR. GEIGER: About that time.

CHAIRMAN BANERJEE: Around that time? And when did the research program sort of finish?

MR. GEIGER: About the same time.

MR. WALLIS: Well, there was comments, for instance, on the use of calcium chloride to simulate the solution of cal sil, give unrealistically high concentration and fails to provide other solutes, such as blah, blah, blah, blah. I'm not aware that anything was done afterwards to change the way in which --

MR. GEIGER: I can't answer that either. I guess the point - we've taken the point from the ACRS that we should do an accounting of the comments, which we have not done a thorough accounting, so perhaps we can leave it at that.

CHAIRMAN BANERJEE: In a more generic way, what this points up is that while you should be commended in having this peer review process, which I think was very valuable, the earlier it can be in a research program in terms of interactions, the better. A different view point.

MR. GEIGER: Well, the sequence is how do you set it up, because you have to have a certain amount of something done so they critique it. Right? And then when they critique it, what do you do, you go back and redo everything again? So it's a dilemma, I think. I'm not sure exactly -- if you have a suggestion how you could work around that problem without doing things twice, and anything --

CHAIRMAN BANERJEE: I guess it's a more iterative process than --

MR. GEIGER: It's difficult to go back and tell LANL that now you've got to redo all these tests and put some pathways --

MR. WALLIS: Well, the good thing is that all the peer reviewers involved in the planning and execution of the program.

MR. GEIGER: I think what we were hoping

is that we had a certain test program out there. We ran it a certain way. We came up with some results, and then the peer review basically did an assessment of whether that was done correctly, was it valuable, or could more have been done? So I guess my main question is, or my consideration is what we have done, is it adequate to inform the question at hand? And you may disagree with me, but at this point, I would say that other than downstream effects and effects on plating out on the core and that sort of thing, which we have not really looked into at all, or plating out on the RHR heat exchangers and so on, where constantly there were temperature fluctuations and things. Other than that, I think we have gotten information that pretty well defines what happens in the sump pool itself, and how it affects the sump strainer. And it points to certain issues that are being dealt with via - by NRR via the industry. I don't know what else I could offer to that. Now, we could --

CHAIRMAN BANERJEE: You've got indications of where the problems are.

MR. GEIGER: I guess what we could do -- the peer review indicates where you could have done more, where problems are. I mean, the statement Dr. Wallis quoted about clogging the strains in four days, well, that's a fact. Right? That's a statement. Now I guess we could look into whether -- we could either do some looking into it to see if we agree with that, or if we could counter it. That may be -- maybe it's not as serious as that reviewer thinks it is. We have not done that, and I guess your point is well taken, that perhaps we should maybe address each --

MR. WALLIS: I think we're spending so much time on this because, in general, we like the idea, I think collectively, of having peer reviews. It provides a useful check on what you're doing. You have some outside eyes, and maybe they see things that you didn't see yourselves.

MR. GEIGER: Yes.

MR. WALLIS: Very useful. And the question which you're asking here is well, now you've got this thing, which is a huge document, much bigger than the original research they're reviewing, what are you doing to do with it? I wouldn't want this to be a bad experience where you decided we'll never have a peer review again. It should be a good experience, and you should benefit from it.

MR. GEIGER: Well, I guess my answer would be to that, is if there was something glaring in the peer review, we should deal with it.

CHAIRMAN BANERJEE: That's nice to hear.

MR. TRAGONING: Well, it's a valid question, certainly, that if points are raised in a peer review, what are you going to do about them? We certainly understand your point.

MR. GEIGER: Let us get back to you on that. Okay?

CHAIRMAN BANERJEE: Yes. Also, let's say some of us submit a report or a paper to a journal where it's peer reviewed, you will get comments like this. Okay? And then before you are able to publish it, you have to actually respond to each comment and do something about it. It's not that you can just pass it off. No, there's no way. All right. Let's go on.

MR. GEIGER: I don't know if there's any point. I just may embarrass myself if I go further on this, because you're not going to agree with some of these things. The comments on the ICET, again - the initial objective of the ICET program was to simulate the sump pools in representative plants, and we tried to cover the bases with the materials that we expected to see. We polled the industry to see what constituents they had and we ran tests on that. And based on that, we identified the compounds that were formed, and it's a very -- it's a seven volume report. It's a huge report, and I don't know what else we could add to that. It's all completed now. I guess we could, if need be, we could resurrect some of the testing, but at this point, I think the ICET program accomplished what it was intended to do, which was to identify the chemical products that are formed --

MR. WALLIS: I've got a question. Will there be chemical products? And the best conclusion from the point of view of what one had to do would be that nothing happened, unfortunately or fortunately, whatever. And, realistically, things did happen, and you did identify some major products, which then led to some work at Argonne. That's fine. It's useful.

MR. KRESS: This could be considered your response to the first three bullets on --

MR. GEIGER: Right. That was the --

MR. WALLIS: But claiming that this thing really simulated what happens in a real sump pool is quite a big step, and I think that quite a few reviewers said this isn't really a real sump pool. Other things are going on.

MR. GEIGER: Well, okay. There's Radiolysis, which we really couldn't --

CHAIRMAN BANERJEE: You could have added

some hydrogen peroxide, for example.

MR. GEIGER: I guess we could have.

CHAIRMAN BANERJEE: Anyway, I think we're only quibbling over the word "sufficient". I think if you say it is the staff's opinion that the tests provide insight into some of the chemical processes --

MR. WALLIS: Some of the major processes, or something.

CHAIRMAN BANERJEE: Yes.

MR. WALLIS: Yes.

CHAIRMAN BANERJEE: I think that would be more modest and more accurate. "Sufficient" is a very strong word there. Perhaps, I mean, you don't want to overstate your case. It doesn't do anybody any good.

MR. GEIGER: Well, I would like to, I guess, not get too deep into this, but I will go back and --

MR. WALLIS: What it told you is that you had to pay attention to chemical effects. That's really what you learned from it. So the question then was what's the next best step?

MR. GEIGER: In the chemical speciation, again I'll state that, the primary purpose of that program was - well, there were actually two of these CNWA programs, one was there were some tests run simulated to identify what type of constituents we should include and the quantities in the ICET test, and to see if temperature was a concern, and should we run at elevated temperatures or fluctuate, so these tests were, I guess, to inform that. And then the follow-on additional chemical speciation prediction was intended to give us a tool, to see if we could find a tool that we could use to then just do an analysis of simple chemistries and predict what the chemical products would be, and so on. So the outcome of that was that it had some benefit, but it was -- the database used in the predictions was not, I guess, wide ranging enough to be able to provide us a tool that we could consider.

CHAIRMAN BANERJEE: The two most knowledgeable reviewers in this direction seem to feel that these programs were rather inadequate for handling the sort of problem that arose here where there was a lot of kinetics, and metastable states and stuff like that. And as far as I know, much of these programs were associated with just thermal dynamics. There wasn't any significant -- so why do you say kinetic prediction there?

MR. GEIGER: Well, because what happened --

CHAIRMAN BANERJEE: They didn't do any kinetics.

MR. GEIGER: They didn't. That's why I'm saying, there wasn't any capability there.

CHAIRMAN BANERJEE: Yes.

MR. GEIGER: Okay? Which is what --

CHAIRMAN BANERJEE: Third would be to determine if these could do thermal dynamics, really. So the word "kinetics" shouldn't even be there. Unless I'm missing something, these quotes couldn't do kinetics.

MR. GEIGER: No, they couldn't.

CHAIRMAN BANERJEE: Yes.

MR. GEIGER: And I guess that was my point in the comment is that they really couldn't, so what was missing, so to predict the rates and so on, how long when these things precipitates actually.

CHAIRMAN BANERJEE: I just want to be enlightened on this a little bit. In the chemical industry, obviously, everything is dependent on kinetics. There are packages around which obviously do kinetics. For example, Dow, one of the consultants you had there, I know uses kinetics packages all the time. Why do you feel that kinetics cannot be done? Is it that it's the specific problem rather than chemical reactors we're talking about?

MR. GEIGER: Unfortunately, I'm not a chemist, and I guess my chemistry goes --

MR. CARUSO: I think part of the big problem is there was not a lot of boric acid data.

CHAIRMAN BANERJEE: Pardon?

MR. CARUSO: There was not a lot of data about boric acid range.

CHAIRMAN BANERJEE: Okay. So the database was -- the kinetics. The kinetic database wasn't there.

MR. KRESS: Well, the first thing you have to do is identify reactions will occur between -- identify your reactants. You may have for a problem like this, you may have 100 reactions going on, and you have to have the reaction rate coefficients. They don't exist. Some of them do, but not all of them, and so it gets very difficult to do a kinetic code if you've got a bunch of species involved.

MR. CARUSO: I think one of the kickers was that the key player was boric acid, and there's not data about boric acid reacting with all these other --

MR. KRESS: There's not, there's not a lot.

MR. KLEIN: That's correct. I think the peer review panel comments thought that there'd need to be development of a whole new database, along with testing to inform that database in order to get to a point where you might have a code that could be more reliable than the existing codes.

CHAIRMAN BANERJEE: But we are talking about bent scale experiments to do kinetics parameters.

MR. KRESS: Oh, yes. Absolutely.

CHAIRMAN BANERJEE: I mean, these are very small, so it's not a very expensive program we're talking about, necessarily.

MR. GEIGER: I don't think it's a trivial job to try and develop a database that includes kinetics, and trying to account for all the different possible materials and combinations of temperatures and pHs.

MR. KRESS: And, in fact, if you're going to have this overall integral predictive model that we talked about, you really have to have that as part of it. That, to me, is almost a prohibitive part of it, that you're not going to be able to put it together.

CHAIRMAN BANERJEE: The impression that is left here is that you can do some thermal dynamics to see what are the equilibrium states, possibly. Unlikely that a system in the short term we build will do anything on the kinetics, so you'll have to depend quite heavily on empirical evidence, what's going on.

MR. KRESS: Which means the integral tests that the industry did have to be pretty good simulations.

CHAIRMAN BANERJEE: Yes.

MR. KRESS: And that's what worries me. I think that's where Cordini - I'm sorry, not Cordini, but a recently departed member from Ohio State had a problem with it.

CHAIRMAN BANERJEE: Rich Denning.

MR. KRESS: Rich Denning, yes.

MR. WALLIS: Well, there's a problem of defining the problem. I mean, some of the viewers said that there are other things going on. I mean, there's fine particulates which you only know about, which are in suspension, which affect the heterogeneous nucleation. Now these are washed down from the building. You don't really know what they are. There are all sorts of things which - small quantities, but finely divided can have a huge effect on the transient precipitation. Just defining the problem is difficult.

CHAIRMAN BANERJEE: This is what the PRT is supposed to do. Right? What state is the PRT in?

MR. GEIGER: The PRT, it's about half - I think the reviewer comments were all tabulated and they went back to the reviewers and they ranked them in significance, and so on, but that's about --

CHAIRMAN BANERJEE: Is the PRT panel the same as the peer review panel, or is it a different panel?

MR. GEIGER: Well, I think a lot of them are the same, yes.

MR. XIAO: No, the PRT was done with the peer review panel.

CHAIRMAN BANERJEE: Okay.

MR. GEIGER: So a lot of the same things appear. They're pretty much parallel.

CHAIRMAN BANERJEE: But it does what Professor Wallis is saying, that tries to give at least their ranking of the phenomena --

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: -- to a first approximation. That would be valuable for us to see at some point, I think. When is that going to be finished, or is it already finished?

MR. GEIGER: Well, it's not -- Rob

Tragoning is working on it, and he sort of --

CHAIRMAN BANERJEE: He said June or something. If I look at my notes, let me just see. Rob's probably gone now, but he said that this might be possible to talk about in June.

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: Is my notes correct on that? So you'd come back to us with a review of the PRT at that time, or what?

MR. GEIGER: Would that be viable? Would you like a presentation on it? Is that --

CHAIRMAN BANERJEE: Well, it seems that where we are is sort of intermediate position right now. We've got these comments, which I think are very valuable. You've got a program which is more or less finished. You're going to respond to this in some way, hopefully, and show -- it's not just the peer review comments. I think you should also have your own comments on this as to where the program has identified things which are valuable which NRR can use, and where some areas are still open issues, which may or may not be resolved by you, but may be resolved by industry. And I think in understanding the importance of these issues, the PRT would be very valuable, because at least you'd have the feedback from the reviewers as to what is important and what is not, which is also essentially what we're asking you to do in some independent way. But if you interact with the PRT, that's fine. I mean, it's not a bad thing at all. You bring your own insights, and the PRT insights, give us some sense of -- because these reviewers are not as close to the problem as you are, in some sense, they're not necessarily - the Dow person is not a nuclear engineer.

MR. GEIGER: Well, this committee participated in some of these PRT, one PRT meeting, I guess the final one where it was basically a brainstorming session type thing, where a lot of ideas were thrown out, and they were all basically captured and put down, and then put in the tables, and it went out for rating and so on, so that -- I think that's --

CHAIRMAN BANERJEE: You got the ratings back now.

MR. GEIGER: Yes, the ratings are back, so that's pretty much where we are. We need to consolidate it, so I'll have to talk to Rob to see when we can finish that.

CHAIRMAN BANERJEE: Give us a hint, what's the most important issue?

MR. GEIGER: Radiolysis seems to play an important role on the radiation exposure, and also the CO2 was brought up. Other than that, I'm trying to think what else would be -- I can't think of anything at this point right now, but I think radiation was one.

CHAIRMAN BANERJEE: Okay.

MR. GEIGER: Of course, I'm sorry, carbon also from - which I mentioned before - lubricants, and then we discussed if there was any freon-type things inside, but typically there aren't any chemicals like that in containment. So I think it was the carbon, the other I mentioned, radiology.

MR. WALLIS: Can we move to the next slide?

CHAIRMAN BANERJEE: Yes, let's go on.

MR. GEIGER: Okay. As far as the recommendation on using the small-scale testing, and I think we covered that. Small-scale tests were done as bench top tests in beakers, and then somebody said well, we need to look at a bigger test. And, also, like the ANL test which his comment was - it consisted of two pages. One, it was to identify the chemical reactions and precipitates, and the other one, they actually did some head loss testing, so I think the scale of the equipment, which was a 6-1/2 inch ID was fairly representative.

MR. WALLIS: This is a very strange conclusion. I mean, this simply says that the facility was adequate. It doesn't say anything about the work was adequate, the results were adequate.

MR. GEIGER: I'm sorry?

MR. WALLIS: It says the facilities were adequate. It doesn't say that the results were adequate.

MR. GEIGER: Oh. Well, the comment --

MR. WALLIS: They may have done no work at all except build a facility.

MR. GEIGER: Well, the comment was - I'm addressing the comment - that we should do small-scale bench testing instead of large tests.

MR. WALLIS: Yes, but if you look at the comments - I mean, I don't want to quote these at you because they'll be on the record, but there's a lot of comments about the limitations of these tests and their results, and the reviews, and you don't put that in your conclusions here.

CHAIRMAN BANERJEE: Well, one of the comments was they could be too conservative, these tests.

MR. WALLIS: Some of them might be too conservative.

CHAIRMAN BANERJEE: Yes.

MR. WALLIS: Right.

CHAIRMAN BANERJEE: So in some sense, it would be to the benefit of --

MR. WALLIS: Using a horizontal screen that collects everything is very conservative.

CHAIRMAN BANERJEE: So you make --

MR. GEIGER: Yes, but at the same point, if you had a vertical screen, you'd have to have a pretty good large screen to observe debris falling or spaulding off, which again, the - is how the industry tests are being done. And their designs have a variety of designs where some are horizontal, some are vertical of this type, so it's --

MR. WALLIS: So is the ANNL testing any use at all for evaluating those industry tests which have completely different geometries?

MR. KLEIN: Certainly it is. I think, keep in mind, the ANL tests were to try and identify some of the head loss consequences, and also to evaluate how changes in things like calcium concentration, or pH, or temperature could potentially impact head loss. The intent of the ANL head loss program was never to take those tests, and then use that to identify the head loss that you might see within a plant-specific situation with a complex strainer, but to have a fundamental understanding of what are important things we should be considering as we go out and observe industry testing.

MR. LEHNING: Could I follow-on to that, Paul? My name is John Lehning in NRR, as well, and no test can simultaneously satisfy all the criteria we might want to have out there, and so these small-scale tests were sort of a first step to let us understand the phenomena that are out there. For instance, had we done larger scale tests, then there would have been other questions, did the stuff transport, did it settle out somewhere in the flume, and how do you know you got the right head loss, and how do you know you'll understand why it wasn't as you predicted. So yes, those are very valuable in helping us understand what happens in the larger tank. And we didn't answer, necessarily, every question you might ask on some of the large-scale tests, but they are very, very helpful.

MR. WALLIS: Well, what they taught you was that calcium phosphate can, under certain conditions, completely block a screen, essentially.

Pressure drop goes up orders of magnitude. They also taught you that hydroxides of aluminum or oxyhydroxide, whatever they are, could completely block a screen under certain conditions, and that's what they taught you, those two things. Really, that's it, because the conditions were not investigated enough for you to say what those conditions had to be in order for this to happen. They just showed that it could be made to happen.

MR. KLEIN: I would argue that they did more than that. I mean, they certainly -- I would argue that they did do that, but I think they did more. They didn't definitively identify every condition, but we did look at a number of different scenarios, and I would especially say with respect to calcium phosphate formation, we looked at a lot of different effects in terms of pH, phosphate dissolution time, cal sil concentration, debris loading timing and sequence, a number of variables that we knew were important in terms of the formation, and we're able to ferret out, in my opinion, some of the more important ones for consideration.

MR. WALLIS: But then let's look at predictability, which was a question raised by quite a few reviewers. Even in the tests where you had no chemical effects, you had this cal sil and Nukon, they did one, which I think was 15 grams of each or something, which they did five or six times, and if you look at those tests, the pressure drop for a certain velocity was ranges from .4 to something like 2.8, or 2.9, or something, and there's a spread, it's .4, .6, 1.2, 1.4. In the same test, there's a range of a factor which is, I don't know, eight or something from the minimum to the maximum, and the data points are scattered all along. This tells you something about repeatability of the tests, even with no chemical effects. Now chemical effects are more whimsical than these physical effects, so presumably repeatability of the chemical tests would need to be investigated. They didn't have any money to do that. They just did the tests. They didn't repeat the whole series.

MR. KLEIN: Well, again, I think those baseline tests were really in line with the types of results that we've seen at PNNL and other places. Variability is not unexpected, especially due to --

MR. WALLIS: But that's with no chemical effects, at all.

MR. KLEIN: Of course, of course. But in some cases, and you have to look at how those effects form, and what the timing of those effects are.

MR. WALLIS: So now you're not going to require - since you know there's a huge variability in the Argonne tests, are you going to require that industry do 20 tests of each condition in order to get the variability and uncertainty, or something? You have to make some decisions based on what you learned, some of which is qualitative. I think you have very interesting questions raised about how you're going to make decisions. Is one chemical effects test going to be adequate, when they did a test and nothing much happened, and they said well, wait a minute, suppose we wait for four days, and gee whiz, something did happen.

MR. KLEIN: This is Research's opinion, and NRR is going to jump in and contradict this opinion. And I shouldn't even say it's Research's opinion, it's my opinion. Let me be particularly honest here. I think there's certain conditions, certain plant-specific conditions that they'll probably be able to make a very easy case that chemical effects are not important either through analysis, or through a limited number of tests. There'll be other conditions, and this is - again, this is my expectation - that will be much more difficult to make that case. Now what strategy we try to go down in that road will depend on a number of considerations. One consideration and one particular path forward would be to do exactly what you've suggested, to try to get a handle on specific uncertainty and variability associated with that particular mix of conditions that's of particular concern to either that certain plant, or a subset of plants. That's one particular possible path forward.

Another path forward would be to look at other mitigative approaches which would alleviate the concerns through other measures, either by removing materials, ensuring that you've got sufficient mitigative strategies even under worse case conditions. There are other ways that you could still tackle and address the problem, other than trying to have a definitive characterization of uncertainty or variability. So which way we go, I think, will depend on many different considerations, and it will be a function of, again, the possible severity concern, the ease of doing some of these other mitigative approaches, and that'll determine how much we really have to understand a particular mix.

MR. KLEIN: And I'd like to add to that - Paul Klein, NRR. There may be cases where a licensee goes out and performs a number of tests to evaluate repeatability. We just observed some testing in New Jersey where they ran a number of repeat tests to try and get an understanding of uncertainty. In other cases, a licensee may do much less testing, but then they need to demonstrate to us that limited number of tests were conservative, that for whatever reason by the amount of precipitate that was added, or other testing decisions that they've made, they've done a bounding-type test.

MR. WALLIS: It's very difficult to show conservativeness, whatever the right word is here. If you look at PNNL, this business of how the stuff comes to the screen, and the order in which it comes, makes a difference of three orders of magnitude in the pressure drop. Now how do you show a condition which is conservative?

MR. KLEIN: Well, I think from a chemical effects perspective, you look at how they modeled what might form within their particular plant-specific environment. In some cases when they apply the WCAP model, they assume that everything that goes in to solution forms a precipitate, and we know from our own testing that that's a conservative assumption. So in some cases, that model drives plants into hundreds and hundreds of pounds of chemical precipitate, and it seems to be a conservative number.

MR. WALLIS: And then if you assume that all of that forms a thin layer on top of the fiber stuff because it goes through the reactor and comes around later, the way that they did at PNNL, then you get a huge pressure drop, which is also unrealistic. I mean, you can push this conservative thing to absurdity extremes, but you don't know how extreme you're being if you don't have a picture of the severability of things.

MR. KLEIN: And I think that's a challenge that we're all facing right now, and what we're seeing from some of the response from industry is that if they have the option, they would prefer not to address uncertainty, they would prefer to take other steps, such as removing a sufficient amount of fiber so that they don't have a covered screen, or evaluating potentially a back-flush system to try and assure themselves if they were to get high head loss situation, they could accommodate that, so there's a lot of work trying to find engineering solutions, rather than quantify the uncertainty.

MR. WALLIS: That appeals to me very much, and I think we've said that in our letter, too; that if you could find a way of getting around this messy problem, that's far better than trying to confront all the uncertainties. I'm just surprised at this slide. It doesn't say anything about the conclusions and the usefulness of the work.

MR. GEIGER: I guess that was not the -- I was just responding to the comments.

MR. WALLIS: Okay.

MR. GEIGER: The conclusions?

MS. TORRES: Well, the objectives were met, but we need to do some follow-up.

MR. WALLIS: Well, the first one, I mean, they did a peer review. Again, objectives should include what kind of response you expect to make to the review, not just the fact that it was done. In that sense, I'm not sure you've really thought through the objectives of the peer review, and the objectives of the peer review should presumably be to get comments, and then respond to them in some way.

CHAIRMAN BANERJEE: Well, I guess one objective would be if the peer review had just blessed everything, it would be the defamation of the program. And if they bless it in general, but have some specific issues, which is, I think, more or less the state, overall. They like the program, but they point out some things that need to be taken care of.

MR. WALLIS: And number three is just a hopeful statement. I think we read from NRR that they have limited value, or whatever the quote was that the Chairman made.

CHAIRMAN BANERJEE: But they qualified that.

MR. WALLIS: Now it's great value?

CHAIRMAN BANERJEE: Plant-specific.

MR. WALLIS: Show me the value, show me one instance of a value.

MR. GEIGER: Well, I think the fact that the statement said that they were limited use, was the fact that they're relying a lot on individual vendor testing --

MR. WALLIS: Show me one - what's the great value? Is there anything that they raise which you found of great value? Give me an example.

MR. KLEIN: If I might jump in - Paul Klein from NRR. I believe it's great value they raised the question of effects of radiation on everything. We did not identify that as part of the ICET or head loss protocol, and it's a valid issue that needs to be addressed.

MR. WALLIS: Thank you.

MR. TRAGONING: Yes, there were several things, and I think the peer review - people have questioned the timing of the peer review, but one thing I will say is that the research that we had conducted provided a good technical basis for most of -- for the peer reviewers to have a better understanding of the types of interactions that can go in these environments. And lacking that, I think the comments wouldn't have been as focused as some of them were, on specific things to address, so there were a number of things. I mean, he brought up the effect of radiation, crud release at the beginning of a LOCA, that was another one that was something that we hadn't really considered in any great detail, so there were a number of specific comments that came out that we thought were particularly important for future, at least, consideration.

CHAIRMAN BANERJEE: So then you can add to your table these are of regulatory significance, we think. All right. Are we done?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: So do you think we could take a break now? So let's go off the record, unless any of you have comments, and we'll reconvene.

(Whereupon, the proceedings went off the record at 10:59:27 a.m., and went back on the record at 11:14:11 a.m.)

CHAIRMAN BANERJEE: So you're going to tell us about the surrogate testing program now.

MR. GEIGER: Yes. Okay. At the request of NRR to help evaluate the use of the Westinghouse surrogate, we asked ANL to do some additional testing on surrogates, actually just one surrogate.

MR. WALLIS: Is this something new that we haven't seen, or is this --

MR. GEIGER: Yes, it is, and I apologize. I just received the report last week from Dr. Shack. It's in the form of a letter report. It should be probably in Adams today, or maybe yesterday. I finally signed it off yesterday.

MR. WALLIS: So they spent their money on this rather than coming here to tell us what they did?

MR. GEIGER: Yes. We had allocated - we found some money after our last meeting with NRR, you expressed some concern about doing some additional testing. We had, at that time, committed to looking at some additional - looking at the surrogate, and doing some other testing with some funds, so we did find some funds to do this.

MR. KLEIN: If I could just add for one

minute here - this is Paul Klein. I think this is an example of the type of research that we might envision moving forward to. In this case, NRR staff had a question about how the WCAP generated precipitate may behave relative to what we had observed in the precipitate that we saw in ICET, and that we generated ourselves in ANL test loop, so we had asked the Office of Research to do two main things for us. One was to try and evaluate the WCAP precipitate, and then the second part, given that earlier tests had compared the head loss behavior in a number of buffers, we observed that sodium tetra borate appeared to be more favorable than some of the other buffers. Since we, at the original time, didn't have enough funds to do the amount of research that we would have hoped for, we went back and requested some additional testing with sodium tetra borate to both evaluate it from the head loss perspective, and also in terms of what we might see with aluminum solubility within sodium tetra borate environments.

CHAIRMAN BANERJEE: So what indications did you have that sodium tetra borate would be better? Was that from the ANL tests?

MR. KLEIN: It was from the earlier ANL tests. They had looked at both sodium hydroxide, trisodium phosphate, TSP, and sodium tetra borate. We spent a lot of time trying to evaluate the TSP, cal sil interaction, since it was important to a number of - about a half dozen or so plants that had that existing combination. We also tried to look quite a bit at sodium hydroxide since about 30 plants had that as a buffering chemical. Sodium tetra borate is currently the least commonly used of the three, but in the earlier round of tests, looked like at certain levels of dissolved aluminum, we saw no head loss response at all, so we wanted to follow-up on that.

CHAIRMAN BANERJEE: Now if I recall, and my memory may not serve, but the sodium hydroxide was a problem when you had aluminum around in large quantities. Right? Or in some quantities. There was no aluminum sodium hydroxide, it was not that much of a problem, was it?

MR. KLEIN: Yes, the sodium hydroxide because it -- currently with plants, it tends to drive you to a higher pull pH, also has greater aluminum corrosion, so it tends to produce more dissolved aluminum within the pull.

CHAIRMAN BANERJEE: Right. But if you take the aluminum out, and this was mainly scaffolding and stuff like that, what was the -- where did the aluminum come from?

MR. KLEIN: It comes from a number of different components, a lot of fan coolers and other type components within containment contain aluminum. Part of the question with plants is trying to have an exact understanding of how much aluminum is in containment. In many cases, plants relied on hydrogen calculations that were known to be conservative on the amount of aluminum in containment, but the degree of conservatism wasn't really known, so unless the plant goes and actually does inventory of aluminum, they tended to default to the higher levels, which in chemical effects space gives you much more precipitate to deal with.

CHAIRMAN BANERJEE: Okay.

MR. GEIGER: Okay. The objectives were to evaluate the head loss performance of the WCAP surrogate precipitates relative to the precipitates generated during the earlier NRC-sponsored tests, as mentioned by Paul.

MR. WALLIS: The question is what's the real -- what's reality?

MR. GEIGER: What's reality? Correct. How do we know we have -- these surrogates actually reflect what's reality?

MR. WALLIS: I think many of the peer reviewers mentioned that what happens in the sump may not be the same as ideal tests.

MR. GEIGER: Actually, with these oxyhydroxides the reality could be anything. It varies so much, and it's the size of the molecules, and so on, and how to coagulate.

MR. WALLIS: The conclusion may be that the Westinghouse surrogate is too conservative. It just blocks everything in sight.

MR. GEIGER: Correct. I guess you went to the end there, so these were --

MR. WALLIS: Well, these are important. We've been saying this all along, that you can't just talk about precipitate. It depends very much on the conditions under which it's made. And this may be a temperature history and all kinds of things that affect how precipitate forms.

MR. GEIGER: So, I mean, these are just some, and --

MR. WALLIS: So what are you going to use? What are you going to use as a standard precipitant?

MR. GEIGER: These tests were done at ANL by Bill Shack, and I just -- these are taken out of the letter report, so --

MR. WALLIS: So he just shows that how you make it influences what it is.

MR. GEIGER: He shows you like different concentrations at the top, and then at the bottom you can see after 20 hours how much had settled out based on --

MR. WALLIS: It says "denser and more compact." Did he do some particle size analysis or something?

MR. GEIGER: These he just --

MR. WALLIS: Just looks at them.

MR. GEIGER: Yes, I mean it's -- you can see it's actually compacted a little bit over the other ones, and there's not much product.

MR. WALLIS: That's the only test he did? He didn't do particle size analysis, or something else?

MR. KLEIN: No, I believe they did more tests than just -- I believe the intent of that slide that shows the pictures was to highlight that if you get -- in this case they were looking at the WCAP surrogate. If you get outside the bounds of the recommendations within the WCAP, you can form something that looks not representative like the other three beakers to the left. So they were trying to assess, in this case, how sensitive the preparation technique was to the precipitate that formed, and I believe they did particle sizing as part of that assessment.

MR. GEIGER: My intent of this presentation is not to present the total research and all the slides that were prepared, just sort of a summary of the research and what the finding --

MR. WALLIS: Well, it's like a lot of these tests, it shows there is an effect. But then if you really wanted to quantify it, you'd probably have to do another research program to quantify it thoroughly.

CHAIRMAN BANERJEE: So remind us briefly about the recommended procedures in WCAP for preparing the surrogates.

MR. GEIGER: Do you have that, Paul? I'm not that much --

CHAIRMAN BANERJEE: Just briefly.

MR. KLEIN: I think in the WCAP they added aluminum nitrate to de-ionized water, and then they add sodium hydroxide which produces precipitate, aluminum hydroxide-type precipitate. The question the staff had was whether that precipitate that formed with that sequence was representative of what we had seen in ICET and what we believe we had reproduced during the ANL head loss testing. Since a lot of the strainer vendors were going to subsequently perform tests using that surrogate as their standard material, we thought it would be important to benchmark that relative to what we had seen during some evaluations ANL did in support of their head loss testing.

CHAIRMAN BANERJEE: These tests you're talking about, what is the objective?

MR. GEIGER: The objective is to see that the particles formed in size and structure are representative of what you would see over a long term, lower concentration chemical reactions in the sump pool.

CHAIRMAN BANERJEE: So they're going to compare this with ICET, or the -- what is the benchmark for comparison?

MR. KLEIN: I think the question that we asked research to evaluate for us was to try and understand the head loss response of these precipitates versus what we had benchmarked as part of the ANL head loss test program, because the ultimate question that NRR had is if we ran - I'm sorry - if industry ran strainer vendor tests with this precipitate and had whatever level of head loss measured, how would we evaluate that relative to the database that had been established in ANL, so we were trying to understand if these precipitates would produce the same degree of head loss as was done previously in the ANL test.

CHAIRMAN BANERJEE: How were the previous - remind me again how the previous tests were done.

MR. KLEIN: In the previous tests at ANL, it was very similar in how the precipitates were produced, but I think the major difference was that sodium hydroxide was added first so that the precipitate formed in an alkaline solution, rather than adding the sodium hydroxide second, such that you were -- essentially had an acidic solution that then became buffer to an alkaline. And some of the earlier tests that ANL had done, we had questioned whether the WCAP sequence of producing a precipitate would form an amorphous precipitate, and so part of their assessment was to look at that. And then also, more importantly, to look at the precipitate in terms of head loss.

MR. WALLIS: Well, that first bullet - is that an ANL conclusion?

MR. GEIGER: That's an ANL conclusion, yes. That's right.

MR. WALLIS: They're concluding that you

can do all this stuff with the surrogate, but it's a test of the surrogate, it's not necessarily a test of reality.

MR. GEIGER: Well, the question was whether these surrogates would give you head loss. It may not be reality compared to the long term.

MR. WALLIS: That wasn't the question they were asked, really. The question was do some tests, wasn't it? Were they asked to assess the validity of using this surrogate at all, which is what the first bullet addresses.

MR. CARUSO: What were they asked to do?

MR. KLEIN: They were asked to evaluate the WCAP precipitate relative to the precipitate that they had generated in their own facilities for -- with respect to how much head loss does this precipitate produce in your test loop.

MR. WALLIS: The first bullet addresses a bigger question than that.

MR. KLEIN: Well, that was -- really, that question was partly addressed in some of the original program, and somewhat in this program, but the other question that was on the table was the WCAP surrogate produce a precipitate that's similar to what we had produced at ANL during earlier testing. If you look at the WCAP, their goal in their surrogate precipitate is to try and simulate the settlement rate and the head loss properties of the precipitate that might form in a post-LOCA environment.

MR. WALLIS: But you see the problem with the first bullet here.

MR. GEIGER: The fine precipitates? Is that what you're --

CHAIRMAN BANERJEE: No, under conditions that might occur in the sump.

MR. WALLIS: Right. I mean, is -- you're saying that there is some other basis for choosing the WCAP parameters for making decisions? Because if it's not representative of precipitates in a sump pool, why do the work at all?

MR. KLEIN: Let me elaborate on that first bullet. I believe the first bullet - one of the things that ANL had done to try and understand what type of precipitate they were forming with their protocol was to look at the response of temperature and pH. And because of that, because of the differences in solubility of crystalline aluminum hydroxide versus amorphous aluminum hydroxide, ANL believed that the precipitate formed by ANL in the head loss loop was an amorphous aluminum hydroxide.

That first bullet, I believe, is intended to say that the WCAP didn't go to that level of demonstration, nor did it claim to. I think the WCAP was trying to say here's a surrogate precipitate that we believe is representative in terms of how quickly it'll settle. And, also, in how it will impact head loss across the debris bed.

CHAIRMAN BANERJEE: So do you want to speak to the second bullet now?

MR. WALLIS: That's the next slide, shows some graph.

MR. GEIGER: So this is the next slide, where they've taken --

MR. WALLIS: They put the stuff in and the pressure drop immediately goes up by two or three orders of magnitude, essentially because the flow rate comes down, the resistance goes up.

MR. GEIGER: Here's the pressure drop in the red line here. I mean, as you can see --

MR. WALLIS: How does this compare with what they had done with the ANL precipitate? How does this compare with a similar experiment using their precipitate? I thought that was the purpose of the test, was to compare this with what they've done.

MR. KLEIN: That's correct. And I'll

address, that Erv.

MR. GEIGER: Yes, go ahead.

MR. WALLIS: Do you have a similar plot for their previous experiment?

MR. KLEIN: I don't know if --

MR. GEIGER: I don't have that.

MR. KLEIN: -- he has that in his package, but if you look at earlier presentations that ANL has provided to the subcommittee, I think you'll see that once you reach the situation where you had precipitate in the loop that was beyond a saturation level, you saw a very rapid head loss increase with time, although nothing quite as dramatic as what's shown in this particular slide.

MR. WALLIS: I don't understand this multiple consecutive additions, because it seems to be in the plot here, it's all in one shot.

MR. GEIGER: Well, that's just - what it is, is that as soon as they added a little bit, before they ever got to the rest of it, they already --

MR. WALLIS: Okay. So it's the first little bit they added that did it?

MR. GEIGER: Yes. I mean, as soon as they had the equivalent of 5 ppm --

MR. WALLIS: So it's 5 percent of 5 ppm

that did it?

MR. GEIGER: No.

MR. KLEIN: NO, I don't think that's correct.

MR. GEIGER: It's not the 5 ppm. This is --

MR. WALLIS: I want to know how does this 5 ppm compare with some experiment they did. I thought they had 100 ppm or something before they got an effect.

MR. KLEIN: Well, it's dependent on the environment.

MR. WALLIS: Yes.

CHAIRMAN BANERJEE: I think it's the 5 ppm, which is the 5 percent of the total.

MR. KLEIN: The 5 ppm should be considered 5 ppm over saturation level such that you had the equivalent of 5 ppm dissolved aluminum in the loop transformed to precipitate. And I think in the earlier tests where they ran for a period of time, you would see either delayed kinetics, or you would run a period of time until you reach some type of saturation level. Once you exceeded that and precipitate started to form, we observed that you did see rapid increase in head loss.

MR. WALLIS: See, if you look at their experiments, there's one here I'm looking at where they add 50 ppm, nothing happened, and then they added some more to bring up to 100 ppm, and suddenly the pressure drop went way up. How does that compare with this sort of experiment here?

MR. KLEIN: That's a sodium tetra borate test. And I think what -- the way to compare that particular result --

MR. WALLIS: NaO3 added. Again, it's aluminum nitrate which then reacts, so it's not the same kind of aluminum as this surrogate, is it?

MR. KLEIN: Well, the way I would interpret that particular test relative to this one, at 50 ppm we don't believe you're forming precipitate in the sodium tetra borate environment, and so you see no head loss response. Once you added 100 ppm, you exceeded the solubility, and you did see precipitate probably greater than the 5 ppm equivalent shown in this particular chart right now. But the head loss response was the same. You saw a very rapid increase in head loss that exceeded their capability.

MR. WALLIS: But quantitatively, how does this compare with the ANL precipitate? Is this a more effective screen blocker? Does it occur at a lower concentration of aluminum? How does it compare with what ANL did, with their aluminum?

CHAIRMAN BANERJEE: This is also ANL.

MR. GEIGER: This is also ANL. Yes, I don't have the --

MR. WALLIS: Are they duplicating a test which is in their report here, ICET-1W?

MR. KLEIN: No, they're not duplicating a test, because --

MR. WALLIS: So how can you compare it with all the stuff that they did?

CHAIRMAN BANERJEE: To answer your question, really.

MR. KLEIN: The WCAP protocol, you generate precipitate, you can do the actual head loss test in a environment that's, for instance, potable water. Okay? The WCAP protocol does not necessarily call for a head loss test using pH buffered borated water environment. Based on that WCAP assertion, the staff had a concern that some of the strainer vendor tests might not be conservative when you just add the precipitate, so the idea was to try and go to the ANL test loop where we had generated head loss in a number of different environments, and add, in this case, a small amount of WCAP surrogate within just a potable water environment to see what the head loss response would be.

The way that we tried to compare it back to the existing database was to look, given some of the solubility information that we knew for some of these environments, like sodium tetra borate, compare how much over a solubility limit we were when we saw head loss response, and then compare the head loss response relative to what we saw with the WCAP surrogate. I think the overall conclusion was that the WCAP surrogate was at least as effective, or maybe more effective at head loss for a given amount of precipitate.

MR. WALLIS: It's very hard to compare with ANL. I mean, they had this 375 ppm, and then they had to cool the stuff down to make a precipitate, but then they were able to make a precipitate with 100 ppm. They didn't really explore the exact conditions necessary to make a precipitate, so I'm not quite sure how you compare this work with the work that ANL had already done.

MR. KLEIN: Some of the follow-on ANL research that you haven't received yet looked at solubilities in sodium tetra borate, and tried to determine at which level you might - once you exceed it, you would start to produce precipitate, and then within the letter report, there's an attempt made to compare that delta on the amount of precipitate that might form over the solubility limit with the amount of precipitate that was added using the WCAP surrogate technique. And when they made that comparison, they thought that the amount of head loss for the WCAP surrogate was at least as high, or it produced head loss equivalent or greater than what was observed with the ANL-generated precipitate.

MR. WALLIS: Why are you talking about sodium tetra borate? I think they just put in lithium hydroxide and boric acid, and NUKON. Then they put in aluminum nitrate solution. I don't see any sodium tetra borate.

MR. KLEIN: There's, I guess, two different tests that I'm referring to. The WCAP surrogate test was not done with sodium tetra borate. The way that we tried to compare the head loss response relates to ANL testing in its sodium tetra borate environment.

MR. WALLIS: So all I learned from this is that the surrogate, the WCAP surrogate can block a screen in very small amounts.

MR. KLEIN: Correct. And that seems to be consistent with what we had seen at earlier ANL testing. There might be time to which you'd form precipitate, but once precipitate began to form within that loop, it appeared like you didn't need a whole lot in order to block the loop.

CHAIRMAN BANERJEE: This had no fiber, nothing.

MR. WALLIS: Yes, it had NUKON --

MR. KLEIN: This test had a NUKON fiber bed.

MR. WALLIS: They filled the fiber bed first, NUKON added.

CHAIRMAN BANERJEE: Oh, NUKON added. Okay.

MR. GEIGER: Now we had more tests planned, but when they ran this first test, it was obvious that it did the job of what they said it would do, so we didn't really go into investigating --

CHAIRMAN BANERJEE: But sodium hydroxide, or sodium tetra borate? What test was this?

MR. KLEIN: The WCAP surrogate test was done without a buffer.

CHAIRMAN BANERJEE: Without a buffer.

MR. KLEIN: Yes. It was to try and run a test that would be similar to what the WCAP protocol would be.

CHAIRMAN BANERJEE: Potable water.

MR. KLEIN: Yes, or they might have used de-ionized, I'm not sure.

CHAIRMAN BANERJEE: All right. So there's not pH buffer.

MR. KLEIN: There is not a pH buffer in this case.

MR. WALLIS: Okay. So it made a smooth top coating, so it made a very thin layer on top of the NUKON.

MR. KLEIN: Last time we put some varnish over it.

MR. WALLIS: We're back to the thin bed-type thing.

MR. ABDEL-KHALIK: Now what is the expected range of concentration? Is 375 ppm within the range of concentration that you would expect during a LOCA?

MR. KLEIN: We would expect 375 to be on the high end of dissolved aluminum concentration in a post-LOCA containment pool.

MR. WALLIS: Only in some plants that have enough aluminum available.

MR. KLEIN: Probably occur with a

combination of a plant that had a significant amount of aluminum, and also a pH controlling chemical, such as sodium hydroxide, that would be on the higher pH end to drive aluminum corrosion. As a benchmark, the staff looked at a license amendment from Fort Calhoun to switch from trisodium phosphate to sodium tetra borate, and as part of that license amendment, they ran the WCAP chemical model with sodium tetra borate to try and estimate what the dissolved aluminum concentration would be in their particular plant specific pool, and I think it was on the order of 22 parts per million.

CHAIRMAN BANERJEE: And if you had sodium hydroxide, how much would it have been, was there such a number?

MR. KLEIN: You could generate that number. They did not, since they didn't have sodium hydroxide, but the --

CHAIRMAN BANERJEE: They had the trisodium?

MR. KLEIN: Yes, they had trisodium phosphate, but they're also a high calcium silicate plant, so they had an incentive to switch from TSP.

MR. ABDEL-KHALIK: So the 375 ppm is a real outlier, as far as expected concentrations. So what would be the probable range that people ought to concentrate on in terms of running experiments, rather than looking at outliers?

MR. KLEIN: Well, I think within the research done at ANL, we tried to look at a range of concentrations from as low as 50, up to as high as 375.

MR. ABDEL-KHALIK: And now Fort Calhoun you said came back with an expected concentration of 22.

MR. KLEIN: Yes.

MR. ABDEL-KHALIK: So what would you do in that situation, if you're outside the range?

MR. KLEIN: Well, in their particular case, given that we had a fair amount of data that indicated at 50 ppm dissolved aluminum with a sodium tetra borate environment, there was no head loss response. We felt comfortable that being less than half of that, they would probably not see a head loss response from a aluminum hydroxide-type precipitate.

MR. WALLIS: I don't know what I conclude from this. Well, I conclude you can do all sorts of different tests with these surrogates. ANL did some stuff, and when they used the Westinghouse stuff, they got a screen clog, but there was quite a different precipitate, clogged in a different way, looked different. I just don't know what this has to do with reality. It seems to me that you can construct all kinds of surrogate experiments, all of which are different, and what do you conclude?

MR. KLEIN: I think the question we raised, and we're trying to address was if licensees were to use the WCAP precipitate in a test that tried to measure head loss across a plant-specific debris load, would that surrogate behave in such a manner that it would produce head loss that was equivalent to what we thought might be a more representative precipitate? And the answer appears to be that very small amounts of the WCAP precipitate induces high head loss.

MR. WALLIS: If they're added after a bed is there.

MR. KLEIN: Well, there's been tests done within industry where they've evaluated the relative timing of addition of chemical precipitate and debris.

MR. WALLIS: Well, this seemed to be the worst case, where you make the bed first, and then you put in the fines, and they make a cake on top of the bed. That's the worst case, it seems. Even in the PNNL, that was the same.

CHAIRMAN BANERJEE: Eventually, they'll recirculate around.

MR. GEIGER: These will recirculate, eventually come around.

MR. WALLIS: They'll recirculate, but these are deposited on the surface of the --

MR. KLEIN: Yes.

MR. WALLIS: -- which I think was the PPNL worst case, too.

MR. KLEIN: Well, when we evaluated calcium phosphate precipitate in the ANL test loop, we saw that the timing of the precipitate addition relative to debris seemed to affect the amount of time that it took for the head loss to reach the high level. But, ultimately, you got to the same answer - the head loss reached a very high level whether you introduced the precipitate first, or the debris first. But it did impact how quickly you achieved that high head loss.

CHAIRMAN BANERJEE: What goes through eventually comes back.

MR. GEIGER: Comes back, yes.

MR. ABDEL-KHALIK: Can we go back to slide three? Okay. Let's look at the first objective. What do you mean by the word "evaluate"? Do you mean compare?

MR. GEIGER: Yes.

MR. ABDEL-KHALIK: So what is the result of that comparison, now that this study has been concluded?

MR. KLEIN: We believe that the WCAP precipitate can produce head loss as effectively as the precipitate that we generated as part of the ANL-sponsored head loss test.

MR. ABDEL-KHALIK: Sure. You put enough of anything, you'll generate head loss that's comparable. The question is, how does it compare at the same concentration?

MR. KLEIN: I think the -- one of the things that we can probably do is provide a copy of the technical letter report, where they tried to make a comparison of equivalent amounts of ANL surrogate to WCAP surrogate. It's difficult to make the exact comparison, but there's some discussion in there that tries to compare the amount of precipitate that you might expect based on being above the saturation amount with the amount that was added in the WCAP. And the conclusion was that the WCAP surrogate for the same amount is as effective at producing head loss, and maybe more so.

MR. WALLIS: This looks a bit like some of these other programs, where as soon as you get an interesting effect, stop the funding. This is something which seems to have been investigated in a very preliminary fashion, and there's no quantitative comparison between WCAP and earlier responsive tests. It's just a sort of qualitative conclusion.

MR. KLEIN: I would agree that this was a very small scale effort.

MR. WALLIS: It's a very interesting result that you do get; and, therefore, it cries for a more thorough investigation.

MR. GEIGER: As to why?

MR. WALLIS: No, as to what happens if you have different concentrations, or different temperatures, or whatever.

CHAIRMAN BANERJEE: I mean, all this just to prevent having to use pH buffered solution experiments, industry experiments.

MR. KLEIN: I think you look at the industry experiments, they tend to be very large-scale tests, and it's probably more difficult for them to run elevated temperature borated water pH buffered. That's not my decision to make, but I guess from my perspective, we need to be able to evaluate what they've done relative to what we know, and so that was the incentive for trying to do at least a small-scale effort to compare the WCAP surrogate to what we had some test knowledge on the surrogate that was produced as part of the ANL test programs.

MR. WALLIS: Can we move to the next slide? I don't understand why we have on our slide a big black square with no numbers at the bottom right-hand side.

MR. GEIGER: The copy machine somehow --

MR. WALLIS: It's very useful to have numbers.

MR. GEIGER: I'm sorry, yes.

MR. WALLIS: Black squares designed to obliterate the number.

MR. GEIGER: The copy that I printed off had numbers, but when I ran it through the xerox --

MR. WALLIS: It's just visible if you're very careful. Okay.

MR. GEIGER: The copy machine made them darker. I apologize.

MR. WALLIS: So here we conclude that the two surrogates are different, the ANL surrogate, and the Westinghouse surrogate are different things. They look different.

MR. GEIGER: Yes. Well, they appear different, but they --

MR. WALLIS: They're different in color.

MR. GEIGER: The question is why the color variation? There was no real explanation. We didn't, you know --

MR. WALLIS: When it says "the layer was impervious", that means no water could flow through it at all?

MR. GEIGER: Correct. They had to drain the water by --

MR. WALLIS: Really blocked up, it was painted. They painted it.

MR. GEIGER: It was painted, yes. They had to open the pressure caps to let the water out.

MR. WALLIS: Westinghouse surrogate paints an impervious layer on top of a fiber bed?

MR. GEIGER: I wish Dr. Shack would be here to address some of these things, but I was planning on him to be here.

MR. KLEIN: I think that's correct. As Erv described, they needed to disconnect the transducer in order to drain the loop.

MR. WALLIS: How did it get to be 2 millimeters thick if there was so little of it?

CHAIRMAN BANERJEE: Is that the total bed thickness?

MR. KLEIN: I think that's what -- he's describing a golden colored layer. I don't know if they tried to separate, or if they could separate the precipitate from the underlying NUKON that probably added part of the golden color to the layer.

MR. WALLIS: Well, I guess we have to go on.

CHAIRMAN BANERJEE: Are these tests now completed?

MR. GEIGER: Yes. Yes.

MR. WALLIS: Number two is getting at my question. I mean, here you've raised a very interesting question, and so you deem that there's no benefit, and stop experiment. This is the whole basis of industry work, is the WCAP surrogates, isn't it?

MR. GEIGER: Well, and what we've done is we demonstrated that if you use the surrogate --

MR. WALLIS: Couldn't industry conclude the same thing?

MR. GEIGER: And they did, yes.

MR. WALLIS: They're not going to use the WCAP surrogate?

MR. GEIGER: About the same time we did

this, they came up with that, so I'm not -- I think they're approaching --

MR. WALLIS: What are they going to use?

MR. KLEIN: Paul Klein, again. I'd like to add some perspective to that second bullet. When we originally started - when we asked research, and then they asked ANL to evaluate the WCAP precipitate, we thought that we would do a series of tests where we incrementally added additional precipitate over time to evaluate the head loss response, much as we had done with some of the earlier testing at ANL. We saw with the WCAP surrogate was that at very low levels of precipitate addition, it produced high head loss, so the feeling was rather than try to evaluate greater than 5 ppm, we knew what the answer would be. To try and evaluate less than 5 ppm, we didn't want to try and split hairs to that level or degree, because it seems like what the results were telling us is that a small amount of precipitate is effective at producing head loss.

MR. WALLIS: It's not ppm that matters, really, it's the total amount of precipitate in the loop, isn't it?

MR. KLEIN: That's correct.

MR. GEIGER: I'm sorry. It was gauged on

a ratio, right - so surface area to what was expected.

MR. KLEIN: When we say 5 ppm, that --

MR. WALLIS: Now this first conclusion is unwarranted. I mean, you could simply say the WCAP aluminum surrogate produces highly unpleasant results. There's no conservative relative to what? I mean, conservative relative to what's really going to happen in the sump? You have no basis for making that statement. And then the second one is extraordinary. I mean, because the surrogate produces an undesirable, highly unpleasant head loss, we're going to stop doing any work with it. That's a strange conclusion.

CHAIRMAN BANERJEE: Is industry continuing to use this surrogate?

MR. KLEIN: Industry went through some initial tests with the surrogate. They've had unpleasant results, as well. I think they've gone back, and there's probably within the five strainer vendors, there's a divergence of opinion on whether to continue with the surrogate, or try another testing approach.

MR. WALLIS: But if this stuff clogs the screen, which it does, it would seem this is a key thing to understand and investigate, and all its dimensions.

CHAIRMAN BANERJEE: So what would industry do, I mean, try to find a surrogate which is less unpleasant?

MR. KLEIN: I will attempt to address that. I hate to speak for industry, so John Butler, if you're still here, and I misrepresent industry, please jump in, but I think industry is looking at this from a broad perspective, and they're trying a number of different things. Some vendors have plants that are able to reduce the amount of fiber, such that they don't get a fiber covered strainer, and that is one way to try and address it, because it seems like one of the consistent test results we've seen is that the precipitate by itself has not been clogging the strainer.

Other people are looking at a number of different options, potentially changing buffers to reduce the amount of precipitate that forms. Westinghouse is trying to go back to some of the initial assumptions in the WCAP model, and look at whether those assumptions were too conservative. For example, aluminum corrosion was all assumed to be commercially pure aluminum, when, in fact, the plants have alloyed aluminum that might have corrosion rates that are much less than commercially pure under the given environment, so there's a number of things that are being looked at in the model to try and remove conservatism, if it's warranted. And then there's a number of test approaches that are looking at can we do other things, such as try to get a bare strainer, or try to incorporate some type of back-flush that would help us get around the chemical effects issue.

CHAIRMAN BANERJEE: But they will not continue to use this surrogate for their testing, or will they continue to use it?

MR. KLEIN: I believe that some of them will continue to use this surrogate.

MR. GEIGER: Well, let me ask - if the surrogate represents the behavior of what you would expect to find in the sump pool, wouldn't it indicate that as soon as you start - you reach the saturation limit, you have precipitates, you would start to affect your fiber bed across the screen? So at levels as low as 5 ppm with the surrogate, you would block the screen. I would consider that to be a conservative test, because let's say, if the real precipitates went at 10 ppm or 20 ppm, then I've under-predicted how much head loss I have.

MR. WALLIS: But this is subject to temperature, as the sump cools down during the long period of recirculation --

MR. GEIGER: More and more precipitates, yes.

MR. WALLIS: Then you get more and more precipitate.

MR. GEIGER: Yes.

MR. WALLIS: And this would seem to indicate that eventually you're going to clog the screen, because the screen has already got its fibers and stuff on it.

MR. GEIGER: Yes.

MR. WALLIS: And now as the stuff cools down, this new precipitate is going to land on top of that fiber layer and make this impervious layer.

MR. GEIGER: So the question is at what stage do I get precipitation of --

MR. WALLIS: Then, of course, because you can't cool the core, things heat up and you dissolve it again.

MR. GEIGER: I'm running for 30 days, do I come down to 60 degrees, or do I just come down to 80 degrees? If I come at 80 degrees, and I still don't have any precipitate, then I'm happy. If, for some reason - but I do get precipitate, then it has to --

MR. WALLIS: Well, isn't this another case where a small experiment done by NRC raises a new key important question?

MR. GEIGER: As to?

MR. WALLIS: Well, it seems to me to be an important question about whether or not you're going to block screens with aluminum precipitates.

MR. GEIGER: Yes. That's a question, I agree. And I think that's why --

MR. WALLIS: Since it's so effective, you seem to decide that there's almost no defense against it, if you've got a fiber layer there already.

MR. GEIGER: What we were saying here is we were looking to see how the surrogate precipitate would behave, to see if it would affect, or how it would compare to pressure drop --

MR. WALLIS: The preliminary conclusion is it's very bad stuff.

MR. GEIGER: And we found that was very bad stuff, so I'm not sure exactly where we would go from here at this point, as far as --

MR. WALLIS: Discontinue the work, right?

MR. GEIGER: Well --

MR. TRAGONING: This is a good example of a case where industry had initiated and was conducting some work. Okay? We had some issues, or some concerns that we had, and one of the concerns was, okay, they're creating a surrogate for this aluminum oxyhydroxide, and the question was how well does that surrogate represent the type of product we saw in the ICET? So there was a very specific concern, so that led to this targeted testing. The targeted testing showed that well, the surrogate probably, in some cases, or in many cases, may not be a very good surrogate of aluminum oxyhydroxide. So the result of that is this information is being used to - and, again, John Butler will, I'm sure, jump in if I mischaracterize - has been used to inform the industry path forward on this in terms of how they want to utilize this surrogate, and how they want to characterize the results they get from this type of testing.

MR. WALLIS: But because of the uncertainties about what's happening in the real sump, you couldn't rule out that something like the WCAP surrogate actually appears in the sump.

MR. KLEIN: That's correct. I think the reason we requested the additional tests from the Office of Research, if a strainer vendor, for example, were to conduct a test with a debris bed, and add the amount of surrogate that's predicted by the WCAP chemical model, and were to achieve a test that demonstrated a head loss that was acceptable, how would the NRR staff review it? And so that's, I think, where we were trying to head with some of this additional targeted research.

MR. WALLIS: What is it that makes this - it's the NaOH that makes these hydrogen --

MR. KLEIN: It's a combination of aluminum and --

MR. WALLIS: It's probably difficult to ban aluminum from power plants, but you can ban sodium hydroxide, couldn't you?

MR. GEIGER: Well, you're going to try to prevent the formation of aluminum oxyhydroxide.

MR. WALLIS: Right.

MR. GEIGER: That's what you need to do, or eliminate fiber, or reduce the amount of fiber, or increase your sump size to the point where the fiber does not totally coat the strainer. And these are all issues that --

MR. WALLIS: Maybe you can coat all the aluminum surfaces, or something?

MR. GEIGER: Well, it depends - that's up to plants, but if you're looking at -- first of all, it was brought up that there was a lot of aluminum in the plant. Well, on the coils, the air coolers and so on, they're at a higher elevation, so they're only going to see this spray for the duration that the containment sprays, a couple of hours, and then there won't be any more. So those will not be submerged continuously in this soup, so we have to look at what's actually on the floor. And I understand that fiberglass insulation has aluminum in it, and all these things. I mean, so there are other sources of aluminum. The question is how much, and how much leaches out? And those are all things that other research has to follow. I mean, what we've proven is that the aluminum surrogate used by Westinghouse, or the recommended surrogate, is very effective at causing head loss on a fiber bed. I don't know how much further we could take it, unless we could prove that well, this stuff doesn't really look like the other.

MR. ABDEL-KHALIK: The point is not whether it is effective at producing head loss. The point is whether it is a true representative of what actually happens. That's what the word "surrogate" means. And go back to slide 3, what is the answer to that question?

MR. WALLIS: Well, whatever question it is, it probably is we don't know.

MR. ABDEL-KHALIK: That's the first objective. The first objective of the research, evaluate the head loss at performance.

MR. GEIGER: Actually, we're evaluating the head loss performance is what we did. Now the question you raised is yes, is it really the right surrogate? That's the question.

MR. WALLIS: What you really seem to have concluded is it's so bad, you don't want to investigate it any more.

MR. GEIGER: Now I would think that industry, Westinghouse would need to determine if this really is -- but is it likely that this form of surrogate could form in your sump?

MR. WALLIS: So you think they're going to come up with a different surrogate?

CHAIRMAN BANERJEE: Well, they got there because they didn't want to use the pH buffered water. Right? I mean, how many options do they have? Not that many.

MR. GEIGER: I think what most of the plants are doing is increasing their screen size to the point that when they take the fiber loading, it does not load.

CHAIRMAN BANERJEE: I don't mean what the plants are doing to cope with the problem, I'm saying what options do they have with the surrogate, not that many.

MR. GEIGER: Not that many, no, unless they want to do the actual chemistry thing.

CHAIRMAN BANERJEE: Yes. So the issue really is, does this give you a high pressure loss over a variety of screen designs? Because I imagine what these people will do is they'll try to change the screen designs and so on, so that you don't get --

MR. WALLIS: So you have some open area.

CHAIRMAN BANERJEE: Yes, have some open area.

MR. GEIGER: Well, that's what they're looking at, is to create so you don't have a uniform coating over the entire mesh or the screen area.

CHAIRMAN BANERJEE: Yes, so you try to take it out.

MR. WALLIS: Well, you could have a design where you actually have a screen which you can open up when you've caught all the fibers on the other screen, then you open it up to let the surrogate through.

MR. GEIGER: I'm not familiar with all the different --

CHAIRMAN BANERJEE: I'm sure there are lots of thinking going on here.

MR. GEIGER: And I think these tests are - and also, the one industry did, because it was pretty much a parallel, about the same time, they came up with the same conclusions that we came up with surrogate.

MR. WALLIS: Are you telling the Commission about this, briefing the Commission about this result? I think you are, aren't you?

MR. KRESS: Today.

MR. WALLIS: Today.

MR. CARUSO: I think that's more of a programmatic briefing, than a technical briefing.

MR. KLEIN: I don't believe that the TA briefing will be to this level of detail.

MR. WALLIS: Well, do we have to tell the Commission about your results?

MR. KRESS: If we think it's a safety issue.

MR. WALLIS: That sort of puzzled me about this whole program, is that we write our letters, and we usually put in a lot of stuff about what you guys have discovered, but really the path shouldn't be through us, it should be directly. And I assume there is a direct path, which is more efficient than going through us.

CHAIRMAN BANERJEE: Well, I guess we add a layer of comment.

MR. WALLIS: The Commission doesn't like surprises, especially from us. It seems to be a significant observation. Am I somehow off base here? I mean, this seems to be a very significant observation, what's being used by industry as a surrogate for aluminum hydroxides is extraordinarily effective at blocking screens. It may well be overly conservative.

MR. KLEIN: Well, I think the precipitate that we generated is effective at blocking screens. The difference in the tests was that there's a solubility component that wasn't involved in this test. In this test you generate the precipitate outside the loop, and you add it to the test. In our test, we added aluminum nitrate within the loop until we exceeded a solubility limit. Once we did that and precipitated a material, we saw very high head loss response.

MR. WALLIS: You also do it by cooling down by the loop.

MR. KLEIN: You could also do it by adding a constant level and dropping the temperate to affect solubility. But the feeling was that equivalent amounts of WCAP precipitate and the that was generated using the ANL protocol both drove head loss high, and maybe the bigger picture was that if you do have these type of precipitates, it doesn't take a whole lot across the debris bed to impact head loss.

MR. WALLIS: Now when industry was here a few months ago talking to us about their experiments, we asked about chemical tests, and they said - some of them said that they had done tests, and the results were unacceptable. Are these -- was it using this kind of surrogate that led to unacceptable pressure drop result?

MR. KLEIN: I missed part of that question. Did you say that they indicated their results were acceptable or unacceptable?

MR. WALLIS: We asked about chemical tests. They said they had done a few chemical tests, and the results gave unacceptable head loss. Was it this surrogate, or some other surrogate for some other particulate matter that gave unacceptable head loss?

MR. KLEIN: It would have been dependent on the particular strainer vendor that you heard --

MR. WALLIS: I don't know which ones they were.

MR. KLEIN: Well, I believe you heard from Allianz Science and Technology, who have used the WCAP surrogate, and they had unacceptable head loss responses.

MR. WALLIS: It was the one we're talking about today.

MR. KLEIN: Yes.

MR. WALLIS: And what are they doing about it?

MR. KLEIN: In their particular case, they are considering a new testing approach that might go back to an ICET-type approach, rather than a WCAP surrogate approach.

MR. WALLIS: They make the chemical in the loop?

MR. KLEIN: They would be -- this is preliminary, so I don't want to provide too many details, but I think they would be looking at what might be generated within a loop given a representative post-LOCA temperature, and pH, and buffered environment.

MR. WALLIS: Well, we could write a letter that said we've told you all of it before, except now we have this new result, which indicates that we were right, that doing research is worthwhile, continuing to do is worthwhile. Write about two paragraphs, and that would be it, because this seems to be a significant result. If you hadn't done this experiment, we wouldn't be reaching some of these conclusions. Very valuable, let's use the word "value" we used before, a very valuable experiment, isn't it, or not?

CHAIRMAN BANERJEE: Well, it's certainly valuable in telling industry not to follow this path, and try to find an alternative.

MR. KRESS: Well, I'm not so sure. If I were interested, and I run a test with this surrogate, and found out that my screen had acceptable flow through it for the ECCS, I would assume I solved my problem. My screen is acceptable to NRC. That's one route to go. Now if they don't get that result, it plugs up their screen, then they've got to do something else.

CHAIRMAN BANERJEE: Which is maybe the sort of test.

MR. KRESS: Yes, and then you've got to evaluate those. But I think this is a valuable result. It says if you use this stuff and get acceptable results, it's acceptable to us. I think that's --

MR. KLEIN: That was the motivation behind our request to try and evaluate the WCAP surrogate within ANL head loss loop.

MR. KRESS: I think that's a reasonable conclusion.

MR. ABDEL-KHALIK: But if the head loss is dependent on the particle size distribution, and this surrogate has a certain particle size distribution that you can get around by redesign so that you can get some acceptable results, what is there to tell us that the actual stuff that's going to deposit will have the same size distribution, so that this design around solution that you came up with would actually work in the real case?

MR. LEHNING: This is John Lehning in NRR. I guess the way that I see it for most of the plants that I think that we've looked at, this was sort of a binary situation, that either you got the chemical effects, and you had a fiber bed, and then you had a really high head loss, or you didn't have the threshold where you're going to get a chemical effect, or you didn't have a fiber bed and you didn't get it, so some of the details of modeling this surrogate with high fidelity would come into play more in the middle ground of is the head loss such and such, or is it twice as much? But if you can say it's either a zero, or it's too high for you to handle, then all those details aren't quite as important, and you're really talking about what are the thresholds at which that effect occurs. And that's what we really need to understand, more so than I think all of the fine points on that surrogate.

Just to follow that a little bit, there may be some plants out there where a certain plant might need more information on that size distribution if they're in a middle ground, and then that would be a valid question to ask for those plants, I think.

CHAIRMAN BANERJEE: So in this surrogate a precipitate is added, not made.

MR. KLEIN: In the WCAP testing protocol, the precipitate is made typically outside of the test, and then added to the test, rather than generating it within the test loop.

CHAIRMAN BANERJEE: Well, the precipitate is made like those little bottles.

MR. GEIGER: Made in a bucket, and poured in, yes.

CHAIRMAN BANERJEE: Poured in.

MR. WALLIS: It says no precipitate was visible in the water?

MR. GEIGER: Correct.

MR. WALLIS: So the stuff that you add is invisible, too? You have a bottle of it, and it's invisible, and you drop it in?

(Laughter.)

MR. KLEIN: When you mix it, you typically see a milky white solution.

MR. WALLIS: You see a milky white solution, but when you put it in the loop, it's so dilute you don't see it? But then it says no build-up was visible. How about this yellow impervious layer that you found? I mean, that's --

MR. KLEIN: That was observed after the test loop had -- well, it had cold, and probably sat overnight and was drained the following day, so it was not --

MR. WALLIS: The yellow layer didn't appear until the next day?

MR. KLEIN: They did not observe a layer at the time the test was terminated, I believe. And that is consistent with some of the tests that ANL had conducted with sodium hydroxide, where they saw a head loss response, but no chemical bed was visible.

CHAIRMAN BANERJEE: Do you have any more slides? Are you done? A little depressing, but --

MR. WALLIS: Maybe there's another surprise.

MR. GEIGER: Well, they did some sodium tetra borate buffer testing just to, I guess, gain some additional information on the solubility at different temperatures.

MR. WALLIS: So you observed very small translucent precipitate particles?

MR. GEIGER: Which eventually - yes.

MR. WALLIS: Am I jumping ahead here, or where am I? I'm on 13, I'm sorry.

MR. GEIGER: You're on 13?

CHAIRMAN BANERJEE: Well, what do you have to say about the previous slide? Go back to where you were.

MR. WALLIS: Yes.

CHAIRMAN BANERJEE: What is the key finding on this slide?

MR. GEIGER: Okay. What we're trying to do here is that we created a solution - two sets. One is, we created these at two different temperatures, and what we did was we started at 10 ppm, and in intervals we added 10 ppm, increased the concentration by 10 ppm, trying to determine when the saturation limit would be reached. And then we had another set at 80, 100, and 120 degrees that we had aluminum concentration of 400 ppm that we let set for a long time.

MR. WALLIS: Were you trying to find out if the sodium tetra borate affects the precipitation of aluminum?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: Then why do you have TSP there in the last --

MR. GEIGER: Oh, did I misspell it? I'm sorry.

CHAIRMAN BANERJEE: But you didn't add trisodium phosphate. Right?

MR. KLEIN: That's a typo.

CHAIRMAN BANERJEE: Oh, that's a typo. All right.

MR. WALLIS: It should be sodium tetra borate, rather than TSP?

MR. KLEIN: STB.

MR. WALLIS: STB. It's rather confusing. STB.

CHAIRMAN BANERJEE: Right. Okay. So we know what --

MR. GEIGER: And we had - this is the test setup, basically. That just shows a sample of the two of them. They were immersed in an oil bath to keep it at constant temperatures.

MR. WALLIS: It's a beaker. And what did you find?

MR. GEIGER: Okay. Observations for the solubility test at the 80 degree F, small translucent particles are formed. And as we increased the aluminum concentration from 50 to 55 percent is when it started to form, and then at concentrations of 90 ppm, the overall solution --

MR. WALLIS: Now when you put in more aluminum, you get less precipitation?

MR. GEIGER: Well, the other -- it actually dissolved a little bit, but --

MR. WALLIS: But the 50, 55 - it looked as if 50 was a critical one, because when you increased from 50 to 55, you got precipitate. When you went up to 90, it went transparent again?

MR. KLEIN: There's a couple of different things that are going on here. At each addition they go to a certain level, and then they hold it over time, and then they look to see if anything drops out of solution, and then they add additional aluminum nitrate over time to increase the level. I think the conclusion was overall the solution was clear, although they did see some precipitation that started to form at a certain level shown here, and then the amount of precipitate that appeared to show up and drop to the bottom increased with increase in concentration.

CHAIRMAN BANERJEE: The solution remained transparent, but that doesn't mean that you didn't find the precipitate.

MR. GEIGER: Right.

MR. KLEIN: Correct.

CHAIRMAN BANERJEE: Okay.

MR. WALLIS: So we should conclude that with TSB, you also get precipitates of aluminum.

CHAIRMAN BANERJEE: Over 50 ppm. Right?

MR. KLEIN: Yes. And that's consistent with their earlier head loss tests where we saw at 50 ppm dissolved aluminum, no head loss response. When they went to 100 ppm, we saw a very rapid head loss increase, so that the objective of these additional tests was to try and evaluate that level between 50 and 100 ppm to see if we could determine a threshold level which we might see precipitation, and also head loss response.

MR. WALLIS: Did you do head loss response with this, too? Presumably not, if it was just mixed in a beaker.

MR. KLEIN: I think some of the follow-on slides are going to discuss at what concentrations they observed head loss.

CHAIRMAN BANERJEE: Go back, don't go so fast. Now you're going to discuss the 100 and 120 cases, what happened there?

MR. GEIGER: Well, these - the ones that were 400 ppm solutions, after about nine days sediments formed and settled, showing that the 120 degree F showed no visible sign of sedimentation by day 20 of the test.

CHAIRMAN BANERJEE: These were still, there was no stirring, nothing.

MR. GEIGER: No, these were all still.

MR. WALLIS: Some of the stuff stays in suspension for days?

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: Well, it's not clear that the precipitate forms immediately either. Right?

MR. GEIGER: Well, they were somewhat cloudy in the beginning, but then --

CHAIRMAN BANERJEE: All of the tests?

MR. GEIGER: Yes, because these were all at 400, yes.

CHAIRMAN BANERJEE: All right.

MR. WALLIS: Why does it stay in suspension? Is it charged?

CHAIRMAN BANERJEE: It could be fine.

MR. WALLIS: It could be fine.

MR. GEIGER: I guess maybe somebody will hit the slide on the --

MR. WALLIS: But now Argonne got blockage of the screen when they couldn't see anything in the water at all.

CHAIRMAN BANERJEE: I guess you could do some light scattering and see what the size of the particles were. Right?

MR. KLEIN: Let me add some additional information here. I think that the objective of these tests was to supersaturate the solution at 400 ppm dissolved aluminum at different temperatures, and then evaluate over time what type of equilibrium dissolved aluminum you might have in the supernate, such that you would try to evaluate, eventually get to the solubility limit at these given temperatures for this system, so that was the objective. And what Erv is describing is that for a number of these, you supersaturated the solution, and precipitate formed and it remained in solution over time, the solution remained cloudy.

MR. GEIGER: And I believe that even after the -- at the end of the test they still had not reached equilibrium. And then we did some head loss tests with STB buffer. And this head loss test was - the loop was filled with de-ionized water, and boric acid and lithium hydroxide were added to the loop. And then sodium tetra borate was added to obtain a pH of 8.3, and the loop was operated for 15 minutes just to mix all the chemicals. And then debris fiberglass NUKON was added, allowed to form on a perforated plate, and the temperature of the loop was raised to 140 degrees F, and the aluminum nitrate was added to provide a final aluminum concentration of 50 ppm.

MR. WALLIS: Is there a theoretical solubility limit under these conditions?

MR. GEIGER: Yes, I'm sure there is, but I didn't include it here.

MR. WALLIS: 50 ppm, are we dealing with a solubility limit, or is it supersaturated, or what is it?

MR. GEIGER: I'm sorry. At 50 ppm, or --

MR. WALLIS: Well, I mean, presumably

there's a theoretical solubility limit for aluminum with this buffer, and the pH and everything, and temperature. I'm just wondering is that around 50, or is it 10, or 5, or 100? Can you identify 50 as being the solubility limit, or is it just --

MR. KLEIN: Well, I think we can give you observations based on testing that ran for a number of days. Whether that's a true solubility limit, it's probably difficult to comment, but in a number of these cases the tests ran on for a long period of days.

MR. GEIGER: Twenty-one days.

MR. KLEIN: Well, even beyond 21 days.

MR. TRAGONING: I wouldn't say these are inconsistent with, because there were solubility calculations that were done, obviously. But there's a number of assumptions that go into those, and as we've seen throughout these experiments and these conditions that they're very sensitive. The actual solubility is very sensitive, the actual conditions you have for a given test, so I wouldn't say they're inconsistent, but I wouldn't want to also say that they are exactly equivalent to either. I mean, 50 was some initial, again, very crude solubility calculations was a pretty good ballpark estimate of when we expected to start seeing --

MR. WALLIS: The peer review talked about other particles, very small particles acting as nucleation centers and things. That would be true if you had a supersaturated solution, but if you're not saturated, then presumably they won't produce precipitates, will they?

MR. GEIGER: If there's nothing nucleate, then yes.

MR. TRAGONING: Unless they provided nucleation sites that wouldn't otherwise be available.

MR. WALLIS: Is this going to be a regulatory requirement that you say as long as you keep your pool under 50 ppm, you don't have to worry about it, if you have to STB you don't have to worry about clogging your screen?

MR. KLEIN: I think we're still trying to determine what the right regulatory guidance should be in the chemical effects area, and our intent is to have some draft guidance by the fourth quarter of this fiscal year.

MR. WALLIS: This is just a very preliminary experiment. Before you have any guidance, you'd want to be pretty sure, presumably.

MR. GEIGER: Well, would we provide this

guidance, or is this something industry would propose, and we would - because it depends on buffers and so on, what they do.

CHAIRMAN BANERJEE: This is true in sodium tetra borate.

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: Is it true if it is buffered in a different way?

MR. KLEIN: No.

MR. GEIGER: No. This is specifically for --

CHAIRMAN BANERJEE: Specific. And what happens if it's buffered differently, is it a lower number?

MR. KLEIN: Well, I think it would depend on the buffering agent.

CHAIRMAN BANERJEE: Well, let's say it was TSP.

MR. KLEIN: With TSP, the concern tends to be more calcium species, formation of calcium phosphate.

CHAIRMAN BANERJEE: Right. Right.

MR. GEIGER: You don't have calcium, yes.

MR. KLEIN: Not aluminum.

CHAIRMAN BANERJEE: And with sodium

hydroxide?

MR. KLEIN: I would want to go back and review the literature on sodium hydroxide before I threw out numbers.

CHAIRMAN BANERJEE: So this 50 ppm is very specific to the system you're talking about.

MR. KLEIN: Yes.

MR. GEIGER: We're trying to evaluate what happens if we held it at 50 ppm for a certain number of days, would we get a precipitation eventually, and the kinetics of it. And so we ran it for 21 days at 80 degrees, with no significant pressure drop in the loop. And after 21 days, we increased the aluminum concentration by 10 ppm increments. After we ran it at 10 ppm for a couple of days, then we increased it. By this time, we were running out of time and budget, but we were trying to see how long we could maintain this loop and test increasing the concentration to see what the -- at what point we would start to create head loss across the screen.

Then what this says here is that "a nominal 70 ppm dissolved aluminum, a notable increase in the pressure drop was observed, indicating that some precipitation was starting to form." And this is a graph of the time history versus -- and shows where we added aluminum at different points.

CHAIRMAN BANERJEE: All this is in the letter report you're talking about.

MR. GEIGER: Yes, it is.

MR. KLEIN: And the head loss response was not inconsistent with what we saw in some of the bench top tests. At 50 ppm, we observed no head loss response at all. It wasn't until we got to a 70 ppm dissolved aluminum concentration, and at that point, head loss started to increase, and they added an additional 10 ppm dissolved aluminum, and it started to climb rather rapidly towards the end of that test.

MR. GEIGER: Small dips in solubility are due to the increased temperature when it was added, so the conclusion, that precipitation kinetics at long-term loop test all suggest that the concentration of 50 ppm aluminum can be maintained in STD and boric acid solutions with a pH of 8.4. And 70 to 80 degree for periods of 20 days. That's the limit of the test. So, I mean, this just sort of informs his plants want to switch to TSB to evaluate.

CHAIRMAN BANERJEE: So how was the -- this test was with aluminum hydroxide being formed in the solution itself. Right?

MR. GEIGER: Well, they added --

CHAIRMAN BANERJEE: Added.

MR. GEIGER: No, they added the --

CHAIRMAN BANERJEE: It was added.

MR. GEIGER: Yes, it was added.

MR. WALLIS: Why was it added?

MR. KLEIN: You're correct. It would have been formed within the loop.

CHAIRMAN BANERJEE: Yes.

MR. GEIGER: Yes.

CHAIRMAN BANERJEE: Okay.

MR. WALLIS: How was it added?

CHAIRMAN BANERJEE: It wasn't.

MR. KLEIN: Aluminum is added as an aluminum nitrate solution.

MR. GEIGER: Nitrate, yes. I'm sorry.

CHAIRMAN BANERJEE: Yes, but the precipitate was --

MR. KLEIN: The precipitate would form in the loop.

MR. GEIGER: It was added at a top, opening at the top.

CHAIRMAN BANERJEE: Okay. All right. So I think -- are you done with your slides?

MR. GEIGER: Yes, I am.

CHAIRMAN BANERJEE: Then we should go for lunch now. Let's take a break until, let's see.

MR. WALLIS: 1:30?

CHAIRMAN BANERJEE: 1:30 sounds good. Okay. We will adjourn.

MR. KRESS: Not adjourn, that means we go home.

CHAIRMAN BANERJEE: Oh, sorry.

MR. ABDEL-KHALIK: Recess.

CHAIRMAN BANERJEE: Recess.

(Whereupon, the proceedings went off the record at 12:28:38 p.m., and went back on the record at 1:33:49 p.m.)

CHAIRMAN BANERJEE: All right. We'll come back into session, and we'll hear from Dr. Krotiuk.

MR. KROTIUK: Now previously at the previous meeting that we spoke about this subject, I had presented some preliminary results of the testing and modeling. All that effort is completed now, so what I want to do is just summarize everything that was done for both the testing and the modeling.

You've seen some of these slides before. This is just to reiterate the objectives of the testing, so I'll talk about the testing first. The objectives were to look at the pressure drop on the sump screen due to debris composition, debris distribution in the bed, fluid temperature effects, and PNNL built a test facility that was able to make in situ bed thickness measurements. They were able to control temperature of the liquid, and we also, I'll talk about this more, were able to measure the constituent masses of the two types of materials in the bed itself. This testing was all done primarily with NUKON and cal sil.

MR. WALLIS: You're presenting PNNL 16-313?

MR. KROTIUK: Yes. I'm presenting the 6917, which is PNNL's effort, and then I'll be going into the modeling effort. I didn't remember the number, I had to look at it.

And as previously indicated, we wanted to do additional testing really to look at the effects of the cal sil addition with mixture with other debris types. We want to address concerns that the ACRS had indicated that they had, and essentially to support regulatory applications. Testing --

MR. WALLIS: You seem to have contracted the test matrix, and they're originally intended to have really thick beds, and higher velocities and things.

MR. KROTIUK: That was changed as time

went on, and we originally had higher velocities, but --

MR. WALLIS: Had 18 inch beds that they originally planned.

MR. KROTIUK: No, we did --

MR. WALLIS: The advantage there is that you can really see the compression of the bed.

MR. KROTIUK: Yes. We did not go that thick, because we were modifying the matrix, and I'll show you the matrix, as we were going along. And it ended up we test in beds that were --

MR. WALLIS: There are some industrial reactor situations where I understand the screen gets completely buried in insulation, so you do get very thick beds.

MR. KROTIUK: That's true, but we looked at -- remember the purpose of the testing was really to come up with a correlation that was defended by the test data, use the data to develop the correlation. If you have too thick of a bed, I don't know how much information you will get, because you may not have much flow. We tried to -- we iterated on the matrix, and I'll show that in a minute. We iterated on the matrix a lot, and you'll see what we finally ended up with, but it wasn't 18 inches, as you indicated. It was thinner than that.

Okay. Let me just describe the test loops a little bit. Basically, at PNNL, we had two test loops, what we called the large test loop was really the test facility where almost all the testing that had the fine data, the bed height measurements, the composition of the bed, that's where we did all the data that we used basically to develop the correlation. But we also had bench top loops, which were smaller diameter loops that we could just run quick little sensitivity studies, so that we could use that as a basis --

MR. WALLIS: Even this is a fairly big loop, though. It's four inches out of six, and it's got a lot of piping and stuff, so --

MR. KROTIUK: Yes. But the 4 inch loop, we were able to -- we didn't have as fine, like we weren't as concerned about having real, real accurate delta P measurements and everything else. It was done for more of a sensitivity study so that we knew how we should maybe modify the conditions in the large loop to get the best data, so that's the purpose of the smaller bench top loop. But you're right, it was still pretty big.

CHAIRMAN BANERJEE: Well, it was less

instrumented.

MR. KROTIUK: It was less instrumented, yes. And data wasn't as good as the large loop. We did a lot of work in the large loop to make sure that the data was as best as we could get.

On the large loop we had temperature control up to 90 degrees C. We were able to measure the bed heights in situ so that we could use an optimal triangulation technique. We were able to pressurize the loop to 150 psi to maintain gaseous solution so we didn't have two-phase flow. That's something that wasn't in the smaller loop. And we also had a filtration system to remove small suspended particles, so we knew what we had flowing in the loop.

Now, let's see. This on the right here is the picture of the test section itself. This is the screen here. We have a clear section here, and the flow basically comes down, so that's this section right here. This is the test section, so the flow would come down. You have delta P measurements across the test section. You have a flow sensor here. This is the filter. It's in parallel to be able to filter out particulates in the loop, the pump, temperature controller, and then it continued like this, and connected up here. The debris in the case of the

NUKON and the cal sil, and other tests that we did, we were able to add it, but in this area on the top of the loop, and we also had an external pressurization, as I said, to 150 psi to prevent gas from coming out of solution.

MR. ABDEL-KHALIK: Now that filter, what size particles can this filter stop? Is it a micron filter?

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: Or a sub-micron?

MR. KROTIUK: Ten micron.

MR. ABDEL-KHALIK: Ten micron. Now in a real case, wouldn't you expect some particles to be within flow much larger than that?

MR. KROTIUK: Yes. This was done just for testing reasons. As you add the debris into the test loop - let me go to here - say we added in this location here, and we started building the test bed in the test section here, we could - even as it's building, there's still suspended particles.

MR. ABDEL-KHALIK: Right.

MR. KROTIUK: So the intent of the filtration was really just to take any of these suspended particles out after we decided to start the test, so that we didn't have - the bed didn't continually start building. We had a set bed that we did the tests on. That's the only reason the filtration system is there.

MR. ABDEL-KHALIK: Okay.

MR. KROTIUK: The testing was done with two types of screens, one was a perforated plate, and one was a five-mesh screen. And then this is - you saw this last time - this is just the way it was mounted in a ring here, so I won't - I don't think I'll pass it around because you saw that previously. The interesting thing is that the specifications on both the plate and the five-mesh screen were specified basically after consulting with NRR, and this is the flow area that they said to be using, and basically the diameter of the holes in the plate, and the square openings in the screen itself.

CHAIRMAN BANERJEE: Let me ask you one thing. The correlation which we've heard about also as it's been developing, remind me if it's only for PWR sump screens, or does it apply to more general in filter - I mean, also in the BWR?

MR. KROTIUK: It is a correlation to calculate pressure drop over a porous medium debris bed, so it could be --

CHAIRMAN BANERJEE: Doesn't matter which

one.

MR. KROTIUK: Doesn't matter which one. As long as you have the properties, that's the key thing, and I'll get to that. You have to know the properties of the debris.

Okay. Let me just go a little bit, describe a little bit how the testing was done in the large loop, because, as I said, that's where most of the data came from. We prepared the debris constituents before it was introduced into the loop, and previously at a previous presentation, Carl Enderlin went through a whole elaborate description of how it was prepared, so I'm not going to go over that again.

For the bulk of our testing, there were a few exceptions to this, we formed the bed at .1 feet per second. We started the flow, we added the debris, and then we kept it at .1 feet per seconds, and built the debris bed on the screen, and basically kept it running for, as you said, about an hour, or seven circulations in this entire loop so that we could build the bed.

MR. WALLIS: Now this -- when you do that, I would think that the big stuff, the long fibers get caught the first time. They don't go through the holes.

MR. KROTIUK: That's probably right.

MR. WALLIS: So then as you build the bed, the top of the bed you're building is going to be made of finer fibers than the bottom.

MR. KROTIUK: That's correct.

MR. WALLIS: And if you have very fine particles, maybe they go around seven times or something before they settle.

MR. KROTIUK: That could definitely --

MR. WALLIS: So you're not producing a homogeneous bed.

MR. KROTIUK: That's absolutely right.

CHAIRMAN BANERJEE: This correlation is supposed to take that into account.

MR. WALLIS: You're going to predict the structure of the bed in your correlation?

MR. KROTIUK: That's what I have attempted to do, with some success. It's not 100 percent successful, but it's some success.

MR. WALLIS: You're going to show this, the sort of concentration of fines and long fibers as a function of depth in some way?

MR. KROTIUK: Yes.

MR. WALLIS: Because there's no

correlation that industry uses that I know of that tries to do this.

MR. KROTIUK: That's correct. In fact, I didn't find --

CHAIRMAN BANERJEE: In fact, you showed us the approach you were taking last time.

MR. KROTIUK: I showed the approach, and it was still being developed at the time.

CHAIRMAN BANERJEE: Well, tell us.

MR. KROTIUK: Yes. One other thing is that we measured the constituent debris mass, for instance, of the NUKON and cal sil before we injected it into the loop, and then after we retrieved the debris bed, we dried it and then measured the weight, so we knew how much was added, and how much was actually deposited on the debris bed.

MR. WALLIS: I'm surprised how much got lost.

MR. KROTIUK: I'm going to discuss that. I have a slide that specifically shows how much got lost.

CHAIRMAN BANERJEE: When were these tests conducted, and when did they finish?

MR. KROTIUK: We finished them in, I think it was early - last September. And they were conducted for about eight months, ten months, something like that, something of that nature.

CHAIRMAN BANERJEE: So they finished September 2006.

MR. KROTIUK: And if we had a debris bed that was composed of say NUKON and the cal sil, we wanted to know how much of that debris was NUKON, and how much of it was cal sil, so we used a post test chemical dissolution technique to dissolve out the cal sil so we could know the weight of the constituents in the bed.

MR. WALLIS: Talking about circulation time, I had a problem with the small scale loop, because they gave the circulation in gallons per minute, and they gave the volume of the loop, and I couldn't make that agree with the circulation time. Maybe someone needs to check that at some time.

MR. KROTIUK: Well, we could check that.

MR. WALLIS: May be a typo or something.

MR. KROTIUK: Yes. Nothing is - you know, could be an error in there. I'll check on that.

MR. WALLIS: That was the small-scale loop?

MR. KROTIUK: No, the seven circulations were for the large --

MR. WALLIS: No, but I was saying the problem I had was with the small scale.

MR. KROTIUK: Okay. Now, again, this was for the bulk of the testing that was done. We took steady-state pressure drop measurements across the debris bed, and we started - we put it through a series of velocities. We changed velocities, and then ran it enough that we got a steady-state pressure. And we started out, as I said, built a bed at .13 per second, and then --

CHAIRMAN BANERJEE: You added the cal sil at that point?

MR. KROTIUK: You added the cal sil at .1 foot per second, so we added all the NUKON and all the cal sil at .1 feet per second, built a bed. Then we would - I'll describe it a little - then we typically would let it circulate for that seven times around the large loop. Then we would open up the bypass filtration system, filter out any of the other particles that may be floating around so that we now have a bed without any - as much as we could --

CHAIRMAN BANERJEE: A stable --

MR. KROTIUK: A stable situation without particles moving around. We closed off the filtration, and then we started doing the testing, and changed the velocities, and measured pressure drop.

MR. ABDEL-KHALIK: So you actually never collected transient data for pressure drop while you're building the bed, during that one-hour period.

MR. KROTIUK: In actuality, pressure drops were continually monitored and recorded, and it's all - it is there on the raw data files, but I didn't use that. I only used the steady-state --

MR. ABDEL-KHALIK: The transient data can help you sort of at least get an idea what the bed structure is, if you know how things change with time.

MR. KROTIUK: That could be interesting. I mean, we have the data. I just didn't really look at it from that point of view.

MR. WALLIS: Now although you cycled the velocity, you didn't seem to get the results that University of New Mexico got with some of their tests, where they go along, pressure drop behaved very nicely with flow rate, and suddenly leap up by a factor of 7 or something.

MR. KROTIUK: I did get a few of those. I'll show you.

MR. WALLIS: You did get a few of those?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: And there was a

hysterisis effect, of course.

MR. WALLIS: They got a bit of hysterisis, but not so much.

MR. KROTIUK: Not so much, yes. But I did get a few of those.

MR. WALLIS: Okay.

MR. KROTIUK: This is just a summary of all the testing that was done. In the large loop, we did testing without debris, with the five-mesh screen and the perforated plate, just to measure the pressure drop for that, for an unclogged or a plate or a screen without any debris on it, so we did five of those tests. We did testing of cal sil only, NUKON only, NUKON and cal sil combined, both in the large and the bench top loops, and you could see that there were a fair number of tests done, 11 cal sil only tests, 90 NUKON only tests, and 45 NUKON cal sil tests. Then we did some additional testing with coatings, primarily in the large loop, but that was a very small number of tests. There was only four tests done.

MR. WALLIS: But you never seem to build up much of a bed of coat.

MR. KROTIUK: I didn't include any discussions under coating, because I wanted to concentrate on the NUKON and the cal sil, but yes, there was not - if you read the report, there was --

MR. WALLIS: Didn't seem to be enough coating to really make a bed.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: But if the coating was involved, wouldn't some of the other insulation be involved?

MR. KROTIUK: You know --

MR. WALLIS: Yes, it would be.

MR. KROTIUK: We came up with a test matrix and investigated the NUKON and the cal sil, and then we started adding some additional testing with the coatings, and basically we ran out of time, and money.

CHAIRMAN BANERJEE: Otherwise, you would have done some with --

MR. KROTIUK: I would have done some - I had like a huge matrix that I was working trying to fill, and one of them was to combine it with the coatings, but I never was able to get there.

CHAIRMAN BANERJEE: What about the paint flakes and things?

MR. KROTIUK: That's included in the coatings. The coatings were done with paint flakes and paint particles.

MR. WALLIS: Now did you sort of blindly follow a test matrix? I remember when you did the cal sil only, for instance, you didn't form a debris bed, but you almost did. I mean, the photos show that you almost got a debris bed.

MR. KROTIUK: Almost.

MR. WALLIS: You've even got a few holes in it.

MR. KROTIUK: Right.

MR. WALLIS: It seems to indicate that with just a little more cal sil, you would have got a debris bed.

MR. KROTIUK: We looked at that to a very, very large degree. We ran, gosh, in the small and the large loop maybe a dozen or so tests where we constantly were increasing the mass of cal sil.

MR. WALLIS: You didn't report those?

MR. KROTIUK: They are in the final report, yes.

MR. WALLIS: And you always got blow-through or something?

MR. KROTIUK: Yes, and I don't remember the number. I think two slides from now I'll show how much we added, and how much was actually deposited. And it came out that only about 10 percent of cal sil on its own was collected.

MR. WALLIS: I think - was it, again, LANL or somebody, was able to make, I think, one bed that was only cal sil.

MR. KROTIUK: Right. That's correct.

MR. WALLIS: So it could happen.

MR. KROTIUK: It could happen, but we tried very hard to build it, and we just were not able to.

MR. WALLIS: Well, there are fibers in cal sil.

MR. KROTIUK: Yes, there are, in somebody's list in 10 percent of the mass is fibers.

MR. WALLIS: You catch the fibers first, then you could build your bed.

MR. KROTIUK: We kept on trying to increase the mass hoping to do that. Okay.

I'm showing the testing that was done, as I said, on the large loop, and I broke it up into Series I and Series II. And so this is just the first testing that we did. Just to identify this, when it says "SO", this was done with a screen, so SO stands for screen only. NO stands for NUKON only, NC is NUKON cal sil. This is the date in the front, as you could see from my little label here.

The thing that I wanted to point, as you indicated earlier, is that, for instance, let's look at this NUKON test right here. We added - wait a minute, that's probably not a good one. Let me just go to a clean one. Okay. We had some problems, just to tell you this, is that the first series tests, like, for instance, we added in this one 165 grams per meter squared. I'd always like to do it grams per meter squared because then that's comparative to whatever kind of loop you were doing testing with. So if you notice that in this situation, we actually ended up with a little bit more mass than we actually put in, and what had happened this one, on the first Series I test, we had ordered a valve, and the valve turned out to be a bad valve, so we had to scrounge - we ordered a replacement valve, but we had to scrounge around for a valve, and the valve we put in had rust particles in it, so some of these tests, as indicated by the asterisks here, had problems because there was some additional rust in the debris bed.

But the thing I really wanted to point out - like, for instance, on this one right here, we added 99 kilograms per meter squared. The bed was only formed at .92, and so that there was only 93 percent of the mass that was added in the NUKON into the loop onto the bed. And when it came to the cal sil for these NUKON cal sil beds, even a small amount of what was added was deposited into the bed.

CHAIRMAN BANERJEE: And you took some of it out with the filter, didn't you?

MR. WALLIS: That's at the end, presumably.

MR. KROTIUK: That's - yes, some of it would have been taken out, but that would really be --

CHAIRMAN BANERJEE: The fine particles.

MR. KROTIUK: The fine particles, yes.

CHAIRMAN BANERJEE: So there was some mass.

MR. KROTIUK: There was some mass that was lost there, yes. We originally would go - actually, we have the data. We didn't look at it too closely, but we actually made some measurements about what went into the filter, and so we actually know how much was deposited in the filter after every test.

MR. WALLIS: And did the cal sil get deposited in the rest of the loop?

MR. KROTIUK: I'm sorry?

MR. WALLIS: Did the cal sil get deposited in the rest of the loop?

MR. KROTIUK: We looked at that during these first tests, the Series I tests. PNNL actually, after doing the NUKON cal sil tests, they actually took parts of the loop apart to look at if there was deposition in the loop itself, and they didn't find - there was some, but not an extreme amount. I don't remember the numbers off-hand.

CHAIRMAN BANERJEE: So where did they go? Where did the mass go?

MR. KROTIUK: Well, there was -- oh, I see what you're saying. Yes. There was deposition in the loop, but - that's a good question.

CHAIRMAN BANERJEE: On the bed, you're only deposited --

MR. KROTIUK: Yes.

MR. WALLIS: If it stayed in the water, you would --

MR. KROTIUK: Yes, some stayed in - let's see. Some had to be deposited, some stayed in the water, and some, when we had the filter system working, some was deposited in the filter. That's the three locations that it could be at. That's a good question.

CHAIRMAN BANERJEE: You never arrived at a mass balance.

MR. KROTIUK: You know, I should have done that, and I didn't include that. That's a good comment.

MR. KRESS: How did you weigh these?

MR. KROTIUK: Okay. You mean the debris bed itself?

MR. KRESS: Yes.

MR. KROTIUK: We took the debris bed out of the test section.

MR. KRESS: The circular, whole thing.

MR. KROTIUK: This whole thing with the debris bed. I'll show pictures of it.

MR. KRESS: Re-weighed it, and subtract out the original weight of the --

MR. KROTIUK: Right.

MR. KRESS: There could be some errors there.

MR. KROTIUK: Yes. There were some errors in there. We had plus or minuses on the measurements, also. But we made sure we dried it.

MR. WALLIS: You dried it?

MR. KROTIUK: I was just going to say, we made sure we dried it.

CHAIRMAN BANERJEE: You would expect the errors, however, to err on the side of having a little more weight than it should have, because some water might be still there. Nonetheless, I mean --

MR. KROTIUK: We put a plus or minus on the measurements.

CHAIRMAN BANERJEE: But still, the effect is large enough that --

MR. KRESS: I think with that way, you're taking two big numbers and subtracting to get a small one, and your measurement errors could multiply that way.

MR. KROTIUK: And especially in the NUKON cal sil bed, remember what I said, is that the way we determined the cal sil mass is that we would measure the weight of the entire bed, then use the chemical deposition - chemical dissolution, I'm sorry, to leach out the cal sil, and then we weighed it again, so the weight of the cal sil was a difference of the total weight minus the weight of the NUKON. So there was errors in there, also.

MR. KRESS: So you could see where the errors in that --

MR. KROTIUK: Yes.

MR. KRESS: But they may not be really important, because what you really want to know is how thick the bed was.

MR. KROTIUK: Yes.

MR. KRESS: And how much was on it.

MR. KROTIUK: Let me just go to the next slide, because this is --

MR. WALLIS: Well, the thicker bed seems to retain higher percentage of the cal sil, in general. It looks like it.

MR. KROTIUK: Yes. Yes. It seemed to be that.

CHAIRMAN BANERJEE: And the mass balance --

MR. WALLIS: Does this enable you to predict how much cal sil will go into the reactor, and not be filtered out?

MR. KROTIUK: I don't know if you could predict it, but it would give you an indication.

MR. WALLIS: Gives you an indication.

MR. KROTIUK: It should give you an indication. This is the Series II test, and this was run with a perforated plate. The first two are plate alone, this is a cal sil only test, these are NUKON only tests, and these are NUKON cal sil tests. This did not have the problem with the dirty valve in it. The valve was replaced for this test, so the measurements are pretty good in terms of what we added, and what we measured.

MR. WALLIS: Even with NUKON you've got only 78 percent in one case left on the bed?

MR. KROTIUK: That's right.

MR. WALLIS: Well, it must depend on how you shredded it.

MR. KROTIUK: You know, that - Carl had done a lot of stuff on shredding it, and yes, shredding it does affect not only deposition, but also affects pressure drop.

MR. WALLIS: PNNL did quite a bit on leave shredding, and blenders, and trying to get the standard --

MR. KROTIUK: Yes. And they basically standardized the way --

MR. WALLIS: What does this have to do with what happens in a LOCA? You don't have leave shredders in a reactor, so what -

MR. KROTIUK: You don't, but -

CHAIRMAN BANERJEE: We visited this one day extensively.

MR. KROTIUK: Yes. We had to shred it in some fashion, and the ultimate aim was to make sure that we shredded it continually the same way so that we could make comparisons, otherwise you can't compare one delta P measurement to another delta P measurement.

MR. WALLIS: Well, how are you going to predict something for - probably depends on which blender you use, what you get for a result.

CHAIRMAN BANERJEE: Or which steam jet, or whatever.

MR. KROTIUK: That's a very valid question, and I don't have --

MR. WALLIS: Unless there's something like surface volume or something that is a factor that correlates everything.

MR. KROTIUK: I have not been able to --

CHAIRMAN BANERJEE: You'll see how many free parameters --

MR. WALLIS: Well, that old NUREG correlation - I forget the number.

MR. KROTIUK: 6224.

MR. WALLIS: That sounds like it. That has some sort of surface area imputed volume, wasn't it, in there?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: It's just a fudge factor.

MR. WALLIS: Well, it gives you

characteristic dimension, that's fine.

MR. KROTIUK: But that's called the SV, specific surface area, and that's a very standard way to measure pressure drop across --

CHAIRMAN BANERJEE: In fact, the way it was adjusted, if you recall, was --

MR. KROTIUK: Yes, absolutely. Because it was like --

MR. KRESS: And they could --

MR. KROTIUK: It was like 600 for a thick bed, and 800, 900 - I'm sorry, 650,000 feet to the minus one for a thick bed, and 800, 900,000 for a thin bed, something like that.

CHAIRMAN BANERJEE: It was all over the place.

MR. KROTIUK: Yes, so I'll address that when I get to the modeling section. Okay.

Just what I wanted to show is that this just summarizes for all those tests, this shows how much of the NUKON that was added was actually ended up being deposited.

MR. WALLIS: 3 percent is right. I guess it is, yes. Well, that's a very small amount of cal sil?

MR. KROTIUK: Yes.

MR. WALLIS: No, it looks like the same experiment done twice. In one case it's 11 percent, another case it's 3 percent.

MR. KROTIUK: Let me just take a quick look.

MR. WALLIS: You've probably got the same amount of NUKON and cal sil added, but you've got much less captured in --

MR. KROTIUK: Yes, they were duplicate testing.

MR. WALLIS: Right.

MR. KROTIUK: But the interesting thing on that is that if you notice, this bed - for instance, these three tests here, they all had the same amount of NUKON and cal sil added, but in this case the bed was built at essentially 20 degrees C, this was about 55 degrees C, and this one was around 80 degrees C. Because we wanted to see the effect of temperature.

MR. WALLIS: Is it the temperature effect, or just bad reproducibility?

MR. KROTIUK: My personal opinion is that it's not related so much to temperature, as it is to just a variance.

CHAIRMAN BANERJEE: Because here it's complicated. You have to take the NUKON out, or the cal sil, and then -- it's a mixed bed. Right?

MR. KROTIUK: Yes, it's a mixed bed. It's NUKON cal sil.

MR. WALLIS: So these are all the tests, these tables?

MR. KROTIUK: These are all the tests that were highly instrumented that were run in the large loop. If you saw --

MR. WALLIS: It doesn't look like many data points on which to build a theory then.

MR. KROTIUK: Yes. I'm not going to argue with that.

MR. WALLIS: I think you have a repeatability problem.

MR. KROTIUK: Yes. I'm not going to argue with you on that.

CHAIRMAN BANERJEE: But building the theory also - I mean, each of these tests you've cycled velocity many times.

MR. KROTIUK: We could only do so much as we could do.

CHAIRMAN BANERJEE: I think Said's point about looking at the build-up of the bed is interesting.

MR. KRESS: That would be interesting to

see, anyway.

MR. WALLIS: Did you monitor the pressure drop as you built up the bed? It said no.

MR. KROTIUK: Yes.

MR. WALLIS: And in their report, they said this was a different question, or something.

MR. KROTIUK: Yes, but it was - I'm going to repeat again. I said this, but maybe it wasn't clear. What we measured in the report was all the stead-state pressure drop values. However, we continually measured pressure drop, and we have it on the data files.

MR. WALLIS: Yes, but there was a statement in the report saying that it was not part of this report, so we don't have it.

MR. KROTIUK: Right. We just didn't report it. That's it.

CHAIRMAN BANERJEE: They have it.

MR. KROTIUK: We have it. We have the raw data. Okay. And just -- this is just to show you samples of the beds that were built. This one on the left here is a very thin bed. It's NUKON only, and the one on the right is a NUKON cal sil bed with indicated loadings, and I notice that the plus or minuses didn't come out, but it's a plus or minus thing.

MR. KRESS: When I envision triangulation, I envision three receivers and time of flight differences. Are you talking about time of flight differences for light waves going to receivers? What is this triangulation --

MR. KROTIUK: Okay. Let me see. Maybe this will -- let me go back just to show you. And I'm going to -- I'm not the one who developed this, PNNL developed this, so I'll try to describe it the best way I know how. Basically, there was a camera set up here, and then a light source here. And the light source would put a grid pattern on the top of the debris bed.

MR. KRESS: A grid pattern?

MR. KROTIUK: A grid pattern. Okay.

MR. WALLIS: Well, that wasn't clear from the description.

MR. KROTIUK: Sorry if it wasn't. So, anyway, --

MR. WALLIS: A grid pattern?

MR. KROTIUK: It puts a grid pattern on it, and then you come up with --

CHAIRMAN BANERJEE: Is that the grid pattern we're seeing there?

MR. KROTIUK: Yes. It's not very -- oh, right. Here it is, see the grid? Right. There it is, that's the grid pattern. And this is actually, as it's expanding and contracting.

MR. WALLIS: If you look at it from an angle or something.

MR. KROTIUK: Yes, we're looking at it from an angle. And you basically standardize it on a zero thickness, and then you could come up with a conversion to different thicknesses. So the net effect of this is that you are actually able to develop --

MR. WALLIS: Could we look back at that?

MR. KROTIUK: Sure. I was just going to show the --

CHAIRMAN BANERJEE: So you have a grid pattern normal to the bed.

MR. KROTIUK: Grid pattern - yes.

CHAIRMAN BANERJEE: So that as the bed moves it blocks.

MR. KROTIUK: Right.

MR. WALLIS: Now you showed some debris on this bed, and it was red and yellow. Why is it two colors? Is the red stuff denser or something, or what? The stuff that's near the screen is redder than the yellow stuff, or does that mean anything there?

MR. KROTIUK: I'm not -- I don't think the color means anything, because if you look at this debris bed here, it's really pretty homogeneous in color.

MR. WALLIS: Well, it's not in depth, though. Homogenous from top to bottom, not from side to side.

MR. KROTIUK: Right. I see what you're saying down here.

MR. WALLIS: Redder there. Anyway, it's elastic.

MR. KROTIUK: It's elastic, yes.

MR. WALLIS: But then it has a set. It's elastic, but if you keep on squishing it, it becomes more squished.

MR. KROTIUK: Right, becomes more squished, but --

MR. WALLIS: Doesn't rebound so much.

MR. KROTIUK: Right.

MR. WALLIS: Like a felt in felting process.

MR. KROTIUK: I'm not familiar with a felting process.

MR. WALLIS: Felt, you start with a lot of fluffy wool and you keep sort of pushing it together until you get a felt, which is the wool squished down to --

MR. KROTIUK: Okay.

MR. WALLIS: That's thinner, but denser. Push the fibers into each other, and it's denser.

MR. KROTIUK: Okay. Sounds similar.

MR. WALLIS: You're doing that here, felting in a way.

MR. KROTIUK: This is just a sample of the readings that we get say for a particular velocity. I didn't indicate it here, but we would have a rim thickness, a body center thickness, an average body thickness because from the optical triangulation grid we could actually come up with a contour, so we actually had a contour of the surface of the debris bed, itself.

MR. ABDEL-KHALIK: How do you maintain constant flow? Is this a PD pump?

MR. KROTIUK: Tom, do you know what kind of pump it was?

MR. KRESS: You need to come to the microphone.

MR. KROTIUK: This is Tom Michener from PNNL. And I don't remember the type of pump so --

MR. MICHENER: I'll repeat that. Tom Michener from PNNL. I wasn't the lead PI on this, but as I recall, that was the type of pump they had on there. They did a pretty good job on maintaining the flow rates, and the pressure, so it wasn't -- where the problem occurred is at the really low velocities. It was hard to keep -- that wasn't our original intent when we hooked up the pump that we used, and so when we went to lower and lower velocities, that became more of a challenge.

MR. ABDEL-KHALIK: So this was a positive displacement pump, control the flow by controlling the pump speed.

MR. KROTIUK: Right. Yes, we controlled the pump speed. Correct.

MR. ABDEL-KHALIK: And you got whatever pressure drop you got.

MR. KROTIUK: Right.

MR. MICHENER: Right. And we had constant read-out of what the flow rate was.

MR. WALLIS: It didn't fluctuate. In some of the other labs, the flow rate fluctuated a fair amount.

MR. KROTIUK: I wanted to make sure that we had a constant flow rate, so that we would adjust the speed of the pump if we had to.

MR. MICHENER: Our loop was highly controllable. We had -- it was pressurized. It wasn't an open loop. We had temperature feedback controls. Like you said, the positive placement pump. We could dial-in what we needed to do, repeatability testing. We had a statistician on board telling us which tests to repeat.

MR. WALLIS: So your report gave a lot of results, didn't have very much of the raw data so we could look at how much the things fluctuate and all that. You just sort of presented a result, and we had to believe it was steady-state. We didn't know how you approached the stead-state and all that.

MR. KROTIUK: That's correct. The report itself is pretty - is what, 500 pages long.

MR. WALLIS: Yes, but it doesn't -- is that the one you gave us?

MR. KROTIUK: There were two reports. There was a report that was 500 pages long, that's the testing report.

MR. WALLIS: Oh, that's not the one I was reading then.

MR. CARUSO: We've got it though, I'm pretty sure.

MR. WALLIS: Well, it's also -- it's one of the other ones that I haven't looked at yet.

MR. KROTIUK: And then there's the 175 pages, which is the --

MR. WALLIS: Gave the results? That's the one I looked at.

MR. KROTIUK: Yes, that's -- there's a lot more data, obviously, in the testing report.

MR. WALLIS: Okay. Thank you.

MR. KROTIUK: This is the -- you've seen this one before, but I just wanted to emphasize something here. This was head loss or pressure drop versus the screen velocity for the -- approach velocity for a case where we had a screen. And what we did here, just to reiterate, is that if we mix the NUKON and cal sil before time and added it to the loop, we had delta Ps as a function of velocity in this area. If we formed the NUKON bed, and then added cal sil after the bed was formed, we actually got a higher pressure drop for equivalent velocity. And then the worst case, which we call the Case-1 case, was that cal sil was added to the loop, and then 30 seconds later, NUKON was added. The NUKON --

MR. WALLIS: The cal sil was still going around the loop.

MR. KROTIUK: Right, so that the cal sil started depositing into the debris bed as it was being formed, and that gave the highest pressure for particulate velocity.

MR. WALLIS: Well, was it as it was being formed? It seemed as if you had a long delay time, so you had the cal sil going around, and the NUKON bed was probably formed before the first cal sil arrived, I would think. It wasn't quite clear.

MR. KROTIUK: When I went down and I was looking at the testing, after you added the cal sil and the NUKON, the debris period, it didn't -- the debris seemed to travel at a different velocity, so you had kind of a long area. It wasn't just all in one area, it was a couple of feet long where there was debris traveling around.

MR. WALLIS: Well, I should think so, yes, because of the velocity turbulence, and the velocity profile and everything. But still, the cal sil was sort of in the loop when you added the NUKON.

MR. KROTIUK: That's correct.

MR. WALLIS: And then it came around later.

MR. KROTIUK: Then it came around later.

MR. WALLIS: So it's rather like the NUKON first experiment.

MR. KROTIUK: It's similar, but that -- we were trying to show the dependence upon injection of it.

MR. WALLIS: This was the most interesting part of the work, to me, and with the most practical implication, and yet it seemed to be stopped here with no more investigation, and that surprised me.

MR. KROTIUK: Let me go through the next slide.

MR. WALLIS: Yes, but there wasn't much more.

MR. KROTIUK: There wasn't much more. Because --

MR. WALLIS: This is a really key thing. I mean, the way in which you make the bed has three orders of magnitude difference, they said, which I agree with on the pressure drop. This has an enormous effect and it's worth understanding. You can worry about getting a 25 percent error in a very controlled correlation or something, but if you have three orders of magnitude depending on how you make the bed, that swamps everything.

MR. KROTIUK: It's a valid point. The one thing is that there's a repeatability when you're doing --

MR. WALLIS: Yes, but that's because you're trying to get a correlation, but if you're trying to represent reality, you've got this huge variation, and you want to know is your reality to the right or to the left.

MR. KROTIUK: Yes.

MR. WALLIS: Where are sumps on this plot?

MR. KROTIUK: I tried -- I'm sorry, what was it?

MR. WALLIS: Where are real sumps on a plot like this?

MR. KROTIUK: And that's a good -- I can't answer that question. I don't know where real sumps are.

MR. WALLIS: So you've got a factor of 1,000 of uncertainty on what's going to happen in a sump?

MR. KROTIUK: Well, one thing I tried to do, and if you read the modeling report, is that - and I'll address this a little bit more later, is that I tried to be able to predict the worse case debris distribution in the bed to come up with an upper limit. And then, similarly, a lower limit for a debris bed, and that would be --

MR. WALLIS: A factor of 1,000 difference?

MR. KROTIUK: It was factor -- I'll show the plots in a while. And I have three sample cases that I plotted here, but there was substantial differences. I don't know if it's -- I don't think it was 1,000. Okay?

MR. WALLIS: You get three orders of magnitude, you have to extrapolate your right-hand data down to the origin, but linear is the worst case. I mean, if it's curved, you get less pressure drop, so that's conservative to use a linear extrapolation. And that does give you a factor about 1,000 between the highest and the lowest. That three orders of magnitude statement in the report is okay.

MR. KROTIUK: Well, when we looked at this Case-1, and something that I did not report on previously when I presented this, when this plot was presented, is that we wanted to see what the distribution of the debris was in the debris bed for this case, so, basically, we did this. This is - I have two cases where we actually -- PNNL developed a methodology where they took a debris bed, they encased -- they injected it with epoxy, hardened it, and then were able to see the distribution of the debris at various locations in the bed. And this is through the bed, flow would have been in this direction, so this is the top of the bed, and this is the sort of --

MR. WALLIS: Arrow on top.

MR. KROTIUK: Right. So you could see what happened here is that in the center region, the bright white lines here are NUKON, and as you could see, these gray areas is the cal sil, and you could see just here and there in this area a little - here's a little blob of cal sil here, but the bottom line of all this is that all -- that the primary distribution of the cal sil is on the surface of the bed. So this is why I - later on when I was developing the model, I said well, let me look at a case where I have cal sil concentrated on the bed surface. That's what I was trying to define as an upper limit.

CHAIRMAN BANERJEE: Now you have Case-1 was the cal sil followed by NUKON. Is this Case-2?

MR. KROTIUK: I'm sorry, this is Case-2. I said the wrong thing.

MR. WALLIS: It's very confusing, because Case-1 is actually Case-4C, and 4B is - one means different things in the --

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: That could have been my fault for labeling.

MR. WALLIS: Very confusing.

MR. KROTIUK: Yes, I labeled it wrong. I'm sorry. That's my fault. That's Case-4. That should have been Case-4. I put the label on incorrectly.

CHAIRMAN BANERJEE: That's Case-4.

MR. WALLIS: But the text is confusing about this, too.

MR. KROTIUK: I'm sorry, but if you look at the labels, the labels are correct. Forget the case, the labels are correct.

MR. WALLIS: We have what's written on the graph.

MR. KROTIUK: And the definition here is correct, complete nucleate formed before adding cal sil. Yes, I did label it wrong.

CHAIRMAN BANERJEE: So what is Case-1A, 1B?

MR. KROTIUK: There were --

CHAIRMAN BANERJEE: Which ones are those?

MR. KROTIUK: Okay.

MR. WALLIS: Those are the ones in the middle, NUKON followed by cal sil.

MR. KROTIUK: NUKON bed formed, then cal sil added.

MR. WALLIS: All the cases on the figure are wrong.

MR. KROTIUK: That is -- I made an attempt to try to label it, and I did a bad job of it.

CHAIRMAN BANERJEE: So Case 1 and 2 on the upper, is the middle.

MR. WALLIS: The bar across the top is correct.

MR. KROTIUK: Yes, the bar across the top is correct.

CHAIRMAN BANERJEE: All right.

MR. KROTIUK: I tried to do some labels to --

CHAIRMAN BANERJEE: That's fine. We can figure it out.

MR. KROTIUK: Okay.

MR. WALLIS: I think it's confusing, because I think in the text there's something like case and run number, or something, and they're different things.

MR. KROTIUK: In the test report, there is a whole section that discusses this, and I read that - this is the report that was done by PNNL, and I took that - this figure from that report. And I tried to annotate it a little, but I believe that it's correctly labeled in the report.

CHAIRMAN BANERJEE: Okay. So in your next figure, it should be really Case-4 that you're talking about.

MR. KROTIUK: NUKON bed formed before - yes, okay.

CHAIRMAN BANERJEE: The next figure, not this. That's okay, yes.

MR. KROTIUK: Case-1. Right.

MR. WALLIS: But when you did the same sort of thing in the large scale, you didn't get these extreme things. You said when you did the large scale head loss result --

MR. KROTIUK: All the testing in the large loop was done with premixed NUKON cal sil.

MR. WALLIS: All done with premix.

MR. KROTIUK: That's correct.

CHAIRMAN BANERJEE: So this case now, which is the --

MR. KROTIUK: The label is correct, forget this.

CHAIRMAN BANERJEE: No, it should be Case-4 Right?

MR. KROTIUK: Yes, should be Case-4.

MR. MICHENER: Tom Michener from PNNL. I

wanted to correct something. I just got a hold of Carl. It was not a positive displacement pump. We do have those, so I had to - that's why I wasn't sure. We were using a centrifugal pump, but we had it attached to a variable frequency drive, so we would just alter the frequency, so that's how we were maintaining it.

MR. KROTIUK: Okay. Thank you. Didn't remember that. This was the second case that we did the sectioning on, and it's similar to the first one in that you could see the center of the region had mainly NUKON, and the surface had the concentration of cal sil.

MR. WALLIS: But when it's premixed, it's quite a complicated problem because the first cal sil that gets to the screen finds no NUKON there and goes right through. And then you begin to build up NUKON bed, which begins to catch some cal sil. But you've still got a lot of cal sil going around the loop, and coming in on top, so modeling that is quite a task, I would think.

MR. KROTIUK: Agreed.

MR. WALLIS: Just because it's premixed doesn't mean it's a homogeneous bed.

MR. KROTIUK: That's correct. And it

definitely was not a homogeneous bed.

CHAIRMAN BANERJEE: But it seems to give you lower pressure drop.

MR. KROTIUK: It gives you the lowest - the premix gives you the lowest pressure drop.

CHAIRMAN BANERJEE: Now why -- these two beds you're showing us for Case-4 and Case-1 and 2, they look somewhat similar. Right?

MR. WALLIS: I think they are.

CHAIRMAN BANERJEE: This case and the next one.

MR. WALLIS: If you add cal sil afterwards --

CHAIRMAN BANERJEE: It doesn't seem to matter what order you --

MR. WALLIS: As long as it's --

MR. KROTIUK: The cal sil seems to collect on the surface of the NUKON bed.

CHAIRMAN BANERJEE: The order doesn't matter.

MR. WALLIS: Well, if you add it and then let it go around the loop, it comes in. In fact, it's coming in later. It's gone around the loop.

MR. KROTIUK: That's right.

CHAIRMAN BANERJEE: But the curious thing is that the two cases, when you reverse the order of NUKON and cal sil --

MR. WALLIS: It doesn't matter, because it's stuck in the loop.

CHAIRMAN BANERJEE: Yes, but then when you premix it, you get a much lower pressure drop.

MR. KROTIUK: Yes. And my hypothesis on that is that the thickness of that layer may be different.

MR. WALLIS: You caught more of the cal sil in the premixing case earlier. You caught some of the cal sil in the bed before it's gone around the loop and deposited on the top.

MR. KROTIUK: So that it's the distribution of the constituents in the bed that is changing.

CHAIRMAN BANERJEE: And what's the circulation time of the loop?

MR. KROTIUK: Well, as indicated earlier, one hour was about seven circulations.

CHAIRMAN BANERJEE: Oh, okay. So by adding the cal sil and then following 30 seconds by NUKON, the cal sil has all just flown through and it's come back one hour later, or one-seventh of an hour later. So the two cases are almost similar.

MR. KROTIUK: There is some similarity.

MR. WALLIS: Now in the real sump, you don't turn on recirculation right away.

MR. KROTIUK: No.

MR. WALLIS: The debris goes down there and settles.

MR. KROTIUK: Yes.

MR. WALLIS: It's quite different, and then it has to be stirred up in order to go through the screen, or is it still in suspension when you turn on the pumps, or what?

MR. KROTIUK: That's a good --

CHAIRMAN BANERJEE: How long after you turn on the pumps?

MR. KROTIUK: Typically, I mean, I did some calculations, and like for one PWR that I remember it was like 1,200 seconds.

MR. WALLIS: 1,200 seconds allows the stuff to settle then.

MR. LEHNING: This is John Lehning from NRR. Some of that debris may end up settled there on the floor of the containment, but it depends, some of that debris may be eroding with time, or some of it may be falling down, it may be blown up.

MR. WALLIS: Washed down by the

recirculation.

MR. LEHNING: That is correct, so it's not clear. I mean, there's a fraction of it doing probably each of those things.

MR. WALLIS: Now what are you going to do about it?

CHAIRMAN BANERJEE: What's the reason your pressure loss is higher in this case than the other case?

MR. KROTIUK: My hypothesis is that you get a larger concentration of the cal sil on the surface, and that's giving you the higher pressure drop.

MR. WALLIS: Well, why?

CHAIRMAN BANERJEE: But why?

MR. WALLIS: Because it's arriving - essentially arriving on top of the NUKON in both cases. That's why I didn't -- it cries for more investigation, really. And just you guys decided to stop.

MR. KROTIUK: I did as --

MR. WALLIS: As soon as you discovered something really significant and important.

MR. KROTIUK: I did as much as I could do.

MR. ABDEL-KHALIK: Is there any way from

your data that you can get local values of porosity or permeability of the bed?

MR. KROTIUK: Not off the top of my head, I can't think of any.

MR. ABDEL-KHALIK: So these SEM images could not determine --

MR. KROTIUK: Wait a minute. Wait a minute. Wait a minute. I guess you can. Yes, you can - just I was thinking about it, from the bed thickness. You know the mass, and you know the thickness of the bed, so you have some idea of the porosity.

MR. ABDEL-KHALIK: That gives you an average value, but I was more interested in local values, and whether that - the variation of the local values is dependent on the order in which the various materials are deposited.

MR. MICHENER: Tom Michener from PNNL. We actually had originally, if there was time, had planned to use some of these pictures to grid up a lattice Boltzman code where you do know the bulk flow, and look at some of the velocities through the bed, and what was happening on there, but there just wasn't time or money to be able to do that.

CHAIRMAN BANERJEE: You have to

reconstruct the porosity anyway.

MR. MICHENER: I mean, you have enough information - we've done that before in similar problems like this one.

CHAIRMAN BANERJEE: You have only a 2-D structure there. You'd have to slice at several locations to --

MR. MICHENER: We could do that.

CHAIRMAN BANERJEE: Yes, sure. I mean, people do that with tumors all the time, they do 64 cuts through it and they reconstruct the porosity.

MR. WALLIS: Then you've got a bed which although it's got more stuff in it, is thinner, too. I mean, the bed thickness is different, B-1 and B-2. That goes the wrong way. You've got more stuff, and yet it's thinner. There's some kind of interplay between the pressure drop, and the compression, and everything else.

MR. KROTIUK: And the distribution of the constituents. And, remember, this is the retrieved bed thickness while it's being tested. That gives you just an idea of what it is. You'd have to know the thickness during testing.

MR. WALLIS: It raises a lot of questions, doesn't it?

MR. KROTIUK: Yes. Okay. What I wanted to just indicate here was, just some basic sensitivity. This is just a plot of three NUKON only tests, pressure drop versus approach velocity for several steady-state velocity points, and this is -since it's all NUKON, it's for three different loadings on the NUKON.

MR. WALLIS: Are these three different tests, or are they cycles within the same test?

MR. KROTIUK: Yes, three different tests. The black, the red, and the green are three different tests.

MR. WALLIS: But why are the three curves in the green? Is that because of cycling?

MR. KROTIUK: Yes. That was cycling.

MR. WALLIS: Well, this isn't a repeatability, this is just a cycling. If you do another experiment with 1.245 NUKON, you might get a different curve from the green curve.

MR. KROTIUK: For the NUKON only test, the repeatability was very high. We were able to duplicate that curve within, I don't know what percentage, but there was a fair amount of repeatability, because we did do that.

MR. ABDEL-KHALIK: If you didn't know

where sort of the balance of the particles ended up, it would be kind of a hit and miss that are duplicating the 1.245 loading. Right?

MR. KROTIUK: Yes, but that's why we measured the weight of the debris bed itself, because we wanted to know what was in the debris bed, not what was added to the loop, what was in the debris bed.

MR. ABDEL-KHALIK: So if they wanted to duplicate the experiment, they would have no idea how much to add to get exactly 1.245.

MR. KROTIUK: Yes, but what we did is that we actually duplicated tests, and we would maybe have 1.245, and 1.251, which was close enough that you could make some conclusions.

MR. ABDEL-KHALIK: Okay. Fair enough.

MR. KROTIUK: You're not going to hit it exactly all the time.

MR. WALLIS: What happens if you do an experiment with .576, and cycle it a few times, and then add some more until you come up to 1.245, does it lie on top of it then?

MR. KROTIUK: I can't answer that question. We didn't look at that.

MR. WALLIS: That's the thing. I mean, they had such an opportunity to explore all kinds of things there. I think they should have been given a bit more carte blanche, rather than being stuck with a test matrix.

MR. KROTIUK: Well, you know, there's arguments on both sides of that, because you have to have a test matrix to begin with. You know, we were doing a lot of testing in the bench top loop trying --

MR. WALLIS: You have a different attitude. In the university we tell a student go and investigate this thing, and then if something interesting shows up, you investigate more what's interesting. You guys have a test matrix, and do this willy nilly, whether it's interesting or not.

MR. KROTIUK: That's not true, because generally, all the testing that we did in the large loop, which is the highly instrumented case, before we did it in the large loop, we did it in the bench top loops.

MR. WALLIS: You explored things.

MR. KROTIUK: And we explored things, and we do some sensitivity studies. That was the intent of the bench top loop, to look at these sensitivities, and then we said okay, now we want to run this case here, and let's put it in the large loop so that we have good instrumentation, and get data.

MR. WALLIS: Okay. So you did do some exploring of things.

MR. KROTIUK: Yes, but it was just beneficial to do it in a bench top loop, because we could turn around what, about three, four tests a day. In the large loop because it was so large, and there was temperature control and all, we could maybe get one, maybe a maximum of two tests a day.

CHAIRMAN BANERJEE: I think your next slide will be of interest.

MR. KROTIUK: Yes, this is what you were talking about.

MR. MICHENER: Just an aside, Bill, real quick. You mentioned earlier the small scale loop was 4 inches, and large scale was 6 inches. And it's not simply a ratio of the square of the radiuses, because the amount of piping that was connected to the large scale was a lot more than for the small scale, so that's why we could do so much more on it, because it was a lot less volume involved.

MR. WALLIS: Still had a pretty long delay time in the small loop.

MR. KROTIUK: Yes, but it was much shorter than for the large loop.

MR. MICHENER: Right.

MR. ABDEL-KHALIK: If I were to just go back one slide, slide 20. If you go to these sets, any one set, does time always go from low delta P to high delta P?

MR. KROTIUK: I'm sorry, repeat that again?

MR. ABDEL-KHALIK: I mean, you're running these, essentially repeating measurements while you're running at the same flow rate. And the question - for the same bed, and the question is, the later experiments, do they always produce higher delta P? I mean, is this a random error, or is it a systematic error always indicating that later experiments give you higher delta P for the same loading?

MR. WALLIS: I think it's the second. Every time you do the experiment, you make commentation.

MR. KROTIUK: You mean for these different cyclings.

MR. ABDEL-KHALIK: Right.

MR. KROTIUK: Okay.

MR. WALLIS: It works its way up. It ratchets its way up.

MR. KROTIUK: Right.

MR. ABDEL-KHALIK: Does it, or are these

random differences?

MR. KROTIUK: There is -- in all the tests that we ran, we would say for the first cycle we would be down here, second cycle would there, and the third would be there, or first, third, second. First was always lowest.

MR. ABDEL-KHALIK: Oh, interesting.

CHAIRMAN BANERJEE: That's probably explainable, isn't it?

MR. KROTIUK: There's a certain amount of - you know, some of the references that I read of define this is what I used at the first compression was non-elastic, and the following compressions were elastic.

MR. WALLIS: But you may still be catching some fines that are going around the loop, too.

MR. KROTIUK: But that's why we had the filtration system, to try to get rid of those fines, as much as we can. We tried to, and that's - there have been some that didn't get captured. I won't deny that, but we tried to eliminate it.

MR. WALLIS: The next one is interesting.

MR. KROTIUK: Yes, that's why I put that here. This is for three loadings, which are -- I tried to look at three cases that had about the same

NUKON loading, but had different cal sil loading. And the thing I wanted to point out here is that the lower cal sil loading had a higher pressure drop than the higher cal sil loading. This is something that was interesting.

CHAIRMAN BANERJEE: But those loading numbers could be fairly inaccurate. Right? Because of the way you determined them.

MR. KROTIUK: There is a plus or minus --

CHAIRMAN BANERJEE: The loading on the bed itself.

MR. KROTIUK: This is the loading on the -- so this is just a mass on the bed divided by the area of the screen.

CHAIRMAN BANERJEE: Right.

MR. CARUSO: Is it possible that lower loading --

MR. WALLIS: Could be a hole in the bed.

MR. CARUSO: Yes, a hole in the bed. I'm just trying to figure out how that would work.

MR. KROTIUK: Well, we continually monitored the bed, because there was a camera on it continually, and we would have known if there was a hole. And definitely, after testing you would have seen that hole.

CHAIRMAN BANERJEE: But you had to remove this stuff chemically.

MR. KROTIUK: Right. There is -- that probably had the greatest inaccuracies, since we were removing the cal sil chemically.

MR. WALLIS: And the top group, the triangles there, presumably that's cycling.

MR. KROTIUK: Yes, it started here, and then the first cycle was here, and then this was a later cycle.

MR. WALLIS: So it lapped off after --

MR. KROTIUK: Yes.

MR. WALLIS: So how are you going to predict all this with a theory?

MR. KROTIUK: With great difficulty, as I said. This is what I tried to relate this phenomena to the distribution of the debris within the bed.

MR. WALLIS: But the business of the .05, .025, .082, that sequence, the only way that could be realistic would be if for some reason the .005 makes itself very, very effective by concentrating in a th in layer.

MR. KROTIUK: That's right.

MR. WALLIS: Why should that happen particularly with that experiment, and not with the others?

MR. KROTIUK: I don't have a good answer for that. I think there's a lot of randomness in this, too.

MR. WALLIS: If you're going to predict these, you have to know.

MR. KROTIUK: Excuse me?

MR. WALLIS: If you're going to predict these, you have to know that.

MR. KROTIUK: Right.

MR. WALLIS: You have to know why it does it.

MR. KROTIUK: Well, I hear what you're saying, but the approach I took, I tried to define maximums and minimums, and tried to bound it, because I thought -- I really believed that the -- looking into the details of what was going on was too complex of a problem for me to handle within the time frame that I had.

MR. WALLIS: Now are these kind of data repeatable? If you do the experiment again with .005, do you get the same result?

MR. KROTIUK: Not necessarily.

MR. WALLIS: No, I don't think you necessarily would.

MR. CARUSO: Go back to some of those shots you had of the surface of this, the debris bed.

MR. KROTIUK: Those?

MR. CARUSO: No, back further, further. That one. I'm just looking at that one on the right there, and it looks like you have some, what I would call rocks. And if you had one of those rocks embedded in the debris bed holding open a pathway, it wouldn't look like necessarily a hole, but it would be opening up a flow path which would give you a larger, or a lower pressure drop than you otherwise might see.

MR. WALLIS: Or a cluster of rocks, maybe.

MR. CARUSO: Or a cluster of rocks like that.

MR. KROTIUK: There's a lot of randomness. I mean --

MR. CARUSO: Ahh.

MR. WALLIS: Are they rocks, or what are they?

MR. KROTIUK: They're not rocks, they're clumps of probably NUKON --

MR. WALLIS: Why are they on top? I would think they would settle out first.

MR. CARUSO: I mean, when you grind up the

NUKON, you're going to get a distribution.

MR. KROTIUK: Right, yes. And, in fact, we did -- distribution measurements, I didn't include it here, but we did make distribution measurements on the size of the cal sil particles, so we actually had distribution measurements, size distribution.

MR. WALLIS: Are those cal sil rocks then?

MR. KROTIUK: I mean, I can't answer that. I don't --

MR. WALLIS: They don't look like fibers.

MR. KROTIUK: It could be clumps of fibers, though. Without looking at it more closely, I mean, we have all these --

MR. WALLIS: You really pulverized the cal sil, didn't you? You wouldn't have rocks like that.

MR. KROTIUK: Yes, but sometimes a conglomerate could --

MR. WALLIS: Conglomerates again.

MR. KROTIUK: Yes, yes.

CHAIRMAN BANERJEE: Well, let's go back to the slide you were at.

MR. KROTIUK: Okay. Let's go to the next one.

CHAIRMAN BANERJEE: How are we doing for time right now?

MR. WALLIS: He's got the whole afternoon.

MR. KROTIUK: I have all afternoon.

CHAIRMAN BANERJEE: Okay.

MR. KROTIUK: Okay. The other --

CHAIRMAN BANERJEE: We do want to finish this afternoon.

MR. KROTIUK: Right, and I'll finish. I'm a little bit less than halfway there.

CHAIRMAN BANERJEE: Okay.

MR. KROTIUK: The other thing that I wanted to look at was temperature sensitivity. So this is when you were saying repeatability. What we tried to do, for instance, we looked at this NUKON, three NUKON cases, and we put the loadings as close as we can. We used the same loadings, and then we measured what was actually deposited in the bed. And you could see the variation, 1.245, 1.251, 1.191. But the difference is that this first bed here was formed at about 20 degrees, this was formed at 54, this was formed at 82. And then we took two NUKON cal sil cases, and with the same loadings that again resulted - same - debris addition that resulted in slightly different loadings on the bed. We formed it at 20 degrees, 54, 82, and then similarly, 20 and 54 for these other loadings. Then we similarly went through what we call LP-1, LP-2, LP-3, because we ran a cycle here, then we upped the temperature, ran a cycle here, upped the temperature, ran a cycle here to try to get some sort of indication of what was going on.

MR. WALLIS: And what happened?

MR. KROTIUK: I'm going to go to the next square. Okay, this is for the NUKON only case, and this is the head loss versus the approach velocity, again for the three cases which are close in terms of loading.

MR. WALLIS: It makes a difference?

MR. KROTIUK: The temperature makes a difference. The lower the temperature, the higher the pressure.

MR. WALLIS: The viscosity effect?

MR. KROTIUK: That's a viscosity effect. This was one of the questions that I think we probably had discussed it at some point.

CHAIRMAN BANERJEE: Yes. The inertial effects here are less if you take an Ergun-type equation.

MR. KROTIUK: Right.

MR. WALLIS: Except that the curves cross down at the .02. That doesn't seem right. Something is wrong down at the extreme left there.

MR. KROTIUK: Yes. It's pretty low, though. There is some slight differences in the loadings, too.

MR. WALLIS: They shouldn't cross.

MR. KROTIUK: Okay. So that's, again, the NUKON, since it was a --

MR. WALLIS: Does this correlate properly with velocity, with viscosity or not? I mean, it doesn't look quite right.

MR. KROTIUK: I'll address that during the modeling. I actually --

MR. WALLIS: I would expect they're more like equally separated, but you've got the two green ones in --

MR. KROTIUK: I reproduced this plot later on with predictions, so if we could just hold off until I get to that point.

MR. WALLIS: I mean, the viscous effect should predominate at lower velocities. Then there should be similar, more of the same at higher velocities. It doesn't seem to be that way around.

CHAIRMAN BANERJEE: We'll flag this as --

MR. WALLIS: He's going to explain it.

MR. KROTIUK: Now for the NUKON cal sil cases, it was somewhat similar for this one, but I have another one which I'll show after this. Again, the loadings are not exactly the same because we added the same mass to the loop, but what was ended up deposited in the bed was slightly different. But, again, you had the - it was formed at three different temperatures. And then let me look at this one, it's kind of interesting.

MR. WALLIS: This one is backwards.

MR. KROTIUK: Yes, this one is backwards. And in this case, the higher pressure was for the higher temperature, and the lower pressure was for the lower temperature, which doesn't fit the theory of viscosity. And what we postulated, this is due to the fact, is that it's the distribution of the debris within the bed. In other words, as you do -- you have your flow temperature, temperature differences, the components in the bed could redistribute, specifically say the cal sil within the NUKON itself.

CHAIRMAN BANERJEE: But did you have any evidence, like core sections, to show that?

MR. KROTIUK: Not of these. Part of the thing was that if we did the sectioning, we couldn't do - measure the masses. It was one or the other, so, you know - because once you did the sectioning, you - once you did, say the cal sil measurements, you couldn't do sectioning, because you destroyed the cal sil, and so we tried to use similar tests. But this was interesting because it showed a different behavior than what I expected to occur just from straight theory Ergun-equation type of unit.

MR. ABDEL-KHALIK: If these had the same exact distribution, and let's say the same conditions, the same geometry within the bed, would the difference on loading account for going from .6 to .69, account for that much difference?

MR. KROTIUK: That's - I mean, I'm sure that that had an effect, also, but we could only get as close as we could get, so yes, that enters into the equation, also.

MR. ABDEL-KHALIK: In this regime, would delta P be proportional to the thickness, or the square of the thickness?

MR. KROTIUK: I think it's proportional to thickness is probably most closest, but it's not --

CHAIRMAN BANERJEE: Well, the pressure gradient doesn't have to be linear, if there is a pressure gradient. I mean, it's really more like pressing on one side.

MR. KROTIUK: You're right. The pressure gradient is not linear, but I'm just thinking of - I'm trying to think of - coming up with some sort of just guidelines, and I would say that it's closer to linear.

MR. WALLIS: You're going to predict all this. Right?

MR. KROTIUK: I'm not going to predict at all. I made a good effort.

CHAIRMAN BANERJEE: You know, again going back to Said's original point, you might get a clue to what's happening if you look at the initial build-up of the bed.

MR. KROTIUK: That's very valid. Again, we have the data. We just didn't look at it.

CHAIRMAN BANERJEE: I mean, different from the other data, in that we reverse the role of temperature, so you're trying to explain this by some form of debris distribution in the bed. However, in this case, the conditions are not all that different from the others, so why is the debris distribution so different? You're getting NUKON followed by cal sil, right?

MR. KROTIUK: This was all done premixed.

CHAIRMAN BANERJEE: This is all premixed.

MR. KROTIUK: This is all premixed.

CHAIRMAN BANERJEE: But what's different

about this than the other ones, which --

MR. KROTIUK: The previous --

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: Loading, the loading was different.

CHAIRMAN BANERJEE: How much?

MR. KROTIUK: Well, let's take a look. That was -- this loading had NUKON of around say .2 kilograms per meter squared, so the last case had about .6 something.

MR. WALLIS: Yes, but it had very different cal sil.

MR. KROTIUK: Different cal sil loadings.

MR. CARUSO: What's the ratio?

MR. KROTIUK: I tried to purposely give a range of ratios to not have --

MR. WALLIS: Well, why didn't - when you were investigating temperature, why didn't you keep the NUKON and cal sil the same?

CHAIRMAN BANERJEE: He did.

MR. WALLIS: He did, but what he got on the screen was different? Is that what happened?

MR. KROTIUK: I mean, like for these three cases, we added the same mass --

MR. WALLIS: What you put in was the same.

MR. KROTIUK: Right. But what --

MR. WALLIS: What you got is very different. I mean, you got four times as much cal sil in one case as in another.

MR. KROTIUK: That's right.

MR. CARUSO: But you don't know where the rest of it went.

MR. KROTIUK: Well, in these case these were the later tests, so some was filtered out, some was deposited in the loop, but probably not a lot.

MR. WALLIS: If I were your professor, I'd say I want more evidence now. I want you to redo that test.

MR. KROTIUK: We would have liked to redo it.

CHAIRMAN BANERJEE: I guess, again, there's a concern with the uncertainty in the measurements of the bed content. Right? Because you were using a chemical --

MR. KROTIUK: Yes. I mean, that's the greatest uncertainty.

MR. WALLIS: You can measure that some time later so you don't immediately get the evidence, which says you now go back and redo the test. These numbers, presumably, came from injection of epoxy and doing --

MR. KROTIUK: No, these numbers came from just weighing the bed --

MR. WALLIS: And then boiling away, and dissolving away the cal sil.

MR. KROTIUK: Dissolving away the cal sil, and then weighing --

MR. WALLIS: Is that done immediately or much later?

MR. KROTIUK: It's done as soon as the beds are dry.

MR. WALLIS: So you get the evidence right away.

MR. KROTIUK: Within a couple of days.

MR. WALLIS: So you could look at it and say this is anomalous, let's do it again.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Just look at the ratio of the cal sil to the NUKON. It's of the order of 10 percent or less. Right? So when you dissolve this stuff away, you're left with maybe 90 percent of the original mass. You're dissolving the cal sil away, right?

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: So you're taking the

difference between two large numbers.

MR. KROTIUK: Yes. That's why I said, it has the --

CHAIRMAN BANERJEE: It's quite tricky, the dissolving process.

MR. WALLIS: And you're predicting it to three significant figures, so --

MR. KROTIUK: Well --

CHAIRMAN BANERJEE: That's just PNNL. Right?

MR. KROTIUK: PNNL and me, both.

CHAIRMAN BANERJEE: You won't pass the buck.

MR. KROTIUK: But you're right, that has the greatest band of error.

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: Okay? Okay, so I'm just going to summarize the testing, then I'll go into the modeling. As I said before, the NUKON only debris bed had relatively repeatable results. And complete - just to make a note here - the complete debris bed, we would generate loadings of .017 kilograms per meter squared. That's the lowest that we did a test at.

As indicated, I can go into it in detail now, is that debris preparation can influence pressure drop; however, loading sequence of the NUKON cal sil debris bed has much stronger influence on pressure drop. As an indicated by one of the graphs that we showed, the increases in cal sil mass in the NUKON cal sil bed did not necessarily yield increases in pressure drop. And I'm attributing this to distribution of constituents in the bed.

This is a picture of a cal sil only debris bed. We added - this is what was added to the loop, 4.352 kilograms per meter squared. I don't remember what that number was, it was pretty high, like 17 grams or something. I don't know. And of that, even with this high loading, we were not able to form a complete bed of cal sil. You could see in these - this is a picture of the debris bed in the test section before it was taken out, and this is a picture after it was taken out. You could see all the holes in the bed itself.

MR. MICHENER: Tom Michener from PNNL. As I recall when we were doing these, what would happen, and it was like it had no strength. It would build up, and then you'd see these little bursts where they go through and a hole would be formed.

MR. WALLIS: You're forcing water through, anyway, with this, so it's got to go somewhere, and it actually makes holes, and you see puffs of stuff coming through?

MR. MICHENER: Yes, you'd see little puffs coming through.

MR. KROTIUK: When I was there for one of the tests, you actually - it was interesting to note, because you'd actually see puffs underneath the debris bed. And, finally, the debris bed does contract and relax with changes of approach velocity. I didn't present any of this, but the screen in the perforated plate testing, they produced comparative results because the flow areas are about the same, and the correlation for the pressure drop for just the screen in a perforated plate agreed with standard, as expected, agreed with standard correlations.

MR. WALLIS: To go back to that cal sil picture, where you see little holes all about the same size, indicate there's one hole in the screen, those little round holes, are they about the size of a screen hole?

MR. KROTIUK: Yes. This is the perforated plate.

MR. WALLIS: They're about the size of a perforated plate hole, those white holes you see there.

MR. KROTIUK: That's probably right.

MR. WALLIS: And one would suspect that they were.

MR. KROTIUK: Yes. I mean, we were postulating that the fibers in the cal sil never got enough to say bridge it, bridged a hole to close it off so that --

MR. WALLIS: Once you've got a hole, it's probably difficult to bridge it, because the stuff gets oriented to go through the hole.

MR. ABDEL-KHALIK: What would be the thickness of this bed, .4 roughly kilograms per square meter?

MR. KROTIUK: It's in the report. I don't remember.

MR. ABDEL-KHALIK: What is that, quarter inch, five millimeters?

MR. KROTIUK: Quarter of an inch, to a half an inch.

MR. WALLIS: Size of the perforation, isn't it?

MR. MICHENER: A little bit more than a quarter of an inch.

MR. KROTIUK: And then my last bullet here just was indicating that in most cases the pressure drop decrease would increase temperature, but the results that the measurement of pressure drop can be affected by the bed history, flow temperature, and ultimately, the distribution of the debris in the bed.

Okay. Do you want to -- I finished under testing. Should I just keep on going?

CHAIRMAN BANERJEE: I think keep going for a little while.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: And we'll take a break. You tell me roughly when you have a turning point.

MR. KROTIUK: Okay. I have a total of 59 viewgraphs, so --

MR. WALLIS: My word.

CHAIRMAN BANERJEE: You won't be able to get through 59.

MR. KROTIUK: No, I won't be able to get through 59.

CHAIRMAN BANERJEE: Get through only the important ones.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: Let's take a half an hour more of this and then we might take a break. Okay?

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: Go on.

MR. KROTIUK: Objectives, I won't go much in detail, except I wanted the model to predict pressure drop at the compression, effects of the bed itself. Motivation - one of the important things was to evaluate the sensitivity to particulate insulation - debris beds composed of particulates and fibers, and regulatory applications, basically to support assessments here at the NRC.

Okay. I have completed this effort right now, and I published a NUREG 1862, which you have a copy of. Okay. Now let's talk about the modeling technique. Something that I sort of described previously. What I tried to do is to come up with a methodology of bounding the upper and lower limit of measurements of pressure drop for a given velocity. So I used a homogeneous debris bed for the lower limit, and a two-control volume for heterogenous debris bed to calculate the upper limit, upper pressure limit, pressure drop. And let me elaborate.

CHAIRMAN BANERJEE: Before you go on, you show us why these are the limits at some point?

MR. KROTIUK: Let's see.

CHAIRMAN BANERJEE: It's not intuitively

obvious.

MR. KROTIUK: Okay. Let me discuss the homogeneous limit. From comparisons of the homogeneous equations, which I'll show in a while, comparing that to test data - say, for instance, if I looked at an all NUKON debris bed, which is basically homogeneous --

CHAIRMAN BANERJEE: Permeated with cal sil.

MR. KROTIUK: Not, let me just talk about NUKON only, first.

CHAIRMAN BANERJEE: Okay.

MR. KROTIUK: So that's completely homogeneous, and correlation would work pretty decently with that, maybe over-predict it a little bit. For the NUKON cal sil debris bed, what I found from doing the calculations, if I assume that the cal sil was evenly distributed in the NUKON, in the debris bed and compared that pressure drop to test data, it always was -- we had test data that was at that value, or above it. It seemed to always indicate a lower limit for the homogeneous mixture of NUKON and cal sil. And this is just comparing --

CHAIRMAN BANERJEE: That's test data.

MR. KROTIUK: This is just comparing with test data.

CHAIRMAN BANERJEE: How do you know the test data was homogeneous?

MR. KROTIUK: Well, no, I did not know that it was homogeneous. When I said that the test data was --

CHAIRMAN BANERJEE: Oh, your model.

MR. KROTIUK: No, I looked at the test data - let's see if I have it.

CHAIRMAN BANERJEE: Let me ask my question a little better.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: Do you have any test data where the cal sil is homogeneously, or nearly homogeneously distributed in the NUKON, after you look at the sections --

MR. KROTIUK: No.

CHAIRMAN BANERJEE: You do not.

MR. KROTIUK: Because we couldn't do a sectioning if we calculated the mass of cal sil. If we calculated the mass -- if we determined the mass of cal sil, we couldn't do a sectioning, because one or the other destroyed the bed, so we never had all the information for one bed.

CHAIRMAN BANERJEE: Right.

MR. KROTIUK: But there are some data from tests that we believe that the cal sil was somewhat evenly distributed in the fiber bed, and in those situations, the measured pressure drop are close to the homogeneous calculations.

CHAIRMAN BANERJEE: I see, and they give you less pressure drop than when you have the two beds separated.

MR. KROTIUK: When I have the situation where I have a concentration of cal sil in a given part of the debris bed.

CHAIRMAN BANERJEE: Is that because they pack better with the -- I mean, each phase, think of it as two phases, almost. Is that the cal sil then forms a more dense bed than it would if it was dispersed in the --

MR. KROTIUK: Yes. And that's what I tried to show by the SEM pictures that I showed earlier, is that if you looked at the bottom of that viewgraph, I gave an indication of the volume percent of the different particulates, the fiber and the particles, I'm sorry, in the sections, and you'll see that in the surface area there was a larger concentration of material than in the center region.

CHAIRMAN BANERJEE: Okay. Fine.

MR. KROTIUK: So based on that thinking, there is four conditions that I postulated could exist. One is that you have a homogeneous, what I call unsaturated particle or fiber bed, or particle and/or fiber bed. In other words, it's all fiber, all particle, or the particles are distributed evenly within the fibers. And this is -- the calculational method for this is a one-volume approach, and I'm postulating that this is the -- would you give you the lower bound delta P for beds with two debris types, and give you a pretty good estimate of the delta P for a bed with one debris type.

MR. ABDEL-KHALIK: What does the word "saturated" refer to?

MR. KROTIUK: Okay. What I'm postulating is that if I look at this case here, the second case, if I have a fiber bed, the amount of particles that could be trapped within that fiber bed has an upper limit, upper practical limit, let's put it that way, but an upper practical limit that would give me the highest pressure drop, and so I'm calling that a saturated particles within the fiber bed.

MR. WALLIS: Depends on how much the fibers are compressed. If the fibers are compressed --

MR. KROTIUK: Yes, right.

MR. WALLIS: -- then you get less void fraction in there to put the particles into.

MR. KROTIUK: Yes.

MR. WALLIS: So the saturation must be somehow related to the compression of the matrix.

MR. KROTIUK: It is related to the compression of the bed, yes.

MR. WALLIS: If you have one that's already saturated, then presumably it can't be compressed any more. Is that right?

MR. KROTIUK: No, it can be compressed, because notice I said practical upper limits. I mean, if you look at an ideal upper limit, it's going to be that you have fibers and the particles completely packing it, and you have no flow. It's just all solid.

MR. WALLIS: Well, there's still some flow passages through the particles.

MR. KROTIUK: Right, yes, but I'm saying a practical upper limit says that there is some flow passages. I'm not looking at a theoretical real --

MR. CARUSO: How much does that depend on the relative geometrical characteristics of the fibers and the particles?

MR. KROTIUK: That's a good question, and I don't really know the answer to that.

MR. CARUSO: I'm not even sure what you would use as the geometrical representation for the particles, because if they're long and thin but break apart, or they deform and wrap around, and if they break when they run into a particle, that -- I mean, there's all sorts of different --

MR. KROTIUK: Yes, I did not try to answer that question, because I thought it was kind of too difficult question to get my hands around, so I tried to look at the test data and try to come up with a methodology that would give me reasonable upper and lower bounds of the test data, and that's why I'm saying practical.

CHAIRMAN BANERJEE: I guess the word "saturated" being used there is not really accurate, because when you think of a saturated porous media, that means all the pores are completely filled by whatever it is, so you're always unsaturated.

MR. KROTIUK: I'm always unsaturated, but I'm at a practical upper limit.

MR. WALLIS: What he means, I think, is that the void fraction available between the fibers is filled with particles.

CHAIRMAN BANERJEE: Well, if it is, then there's no flow.

MR. KROTIUK: Then there's no flow.

MR. WALLIS: There's gaps between the particles.

CHAIRMAN BANERJEE: Oh, yes, so it's not saturated.

MR. WALLIS: Yes, it is. You've got as many particles in there as can get in there.

CHAIRMAN BANERJEE: Without deforming the fiber.

MR. WALLIS: Yes, without deforming the fibers or the particles.

CHAIRMAN BANERJEE: Yes, or the particles.

MR. ABDEL-KHALIK: But like Ralph said, this is a geometry problem. It depends entirely on geometry.

MR. KROTIUK: But it's the form of --

MR. CARUSO: Right. I mean, it's like I'm going back to my heritage, a bowl of spaghetti and meatballs, how does the sauce get through? It all depends on what kind of pasta you use.

MR. KROTIUK: I like ziti.

(Laughter.)

CHAIRMAN BANERJEE: But why do you need to make this distinction? More to the point, I mean --

MR. KROTIUK: What I was trying to do is --

CHAIRMAN BANERJEE: There's no practical way of determining if there's a saturation limit, because --

MR. KROTIUK: Right, and so this - what I used is the test data to try to come up with this practical upper limit.

MR. WALLIS: I think you could do it geometrically. Forget about cal sil, you could take very fine glass beads, and you could take fibers, and you could geometrically work out what --

CHAIRMAN BANERJEE: If they don't deform. You could take needles and spheres, and you could find a --

MR. WALLIS: Take spaghetti and spheres.

CHAIRMAN BANERJEE: No, spaghetti and spheres make it very difficult.

MR. WALLIS: Well, it's still doable, at least experimentally.

CHAIRMAN BANERJEE: Maybe, but not theoretically.

MR. WALLIS: Caviar and spaghetti, if you ever want to eat something like that.

CHAIRMAN BANERJEE: Well, let's do this. Let's run through this, because your definition of saturated is --

MR. WALLIS: We're going to run through it, right?

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: -- going to make it difficult.

MR. KROTIUK: Just remember, you're right. Saturated doesn't mean - really it's a practical upper limit. I was trying to point a terminology.

CHAIRMAN BANERJEE: Saturated porous media has a clear meaning.

MR. CARUSO: How about closely packed?

MR. KROTIUK: Well, not fully packed. You don't want to say - if it was fully packed, it would mean there's --

CHAIRMAN BANERJEE: He said closely packed.

MR. CARUSO: It's like crystalline packed structures, crystals.

CHAIRMAN BANERJEE: I doubt if there is a practical upper limit. If you go on, let's say you took a bed which you call saturated and you compressed it --

MR. WALLIS: You can't compress it, because the particles are not compressible.

CHAIRMAN BANERJEE: Well, but the -- oh, are you meaning that if you -- so if you're saying that there's a practical way of determining this, you go on putting particles in a bed until you can't compress it any more. Is that how you're --

MR. KROTIUK: That's what -- well, part of it, because there is a calculation of compressibility. I mean, as you --

CHAIRMAN BANERJEE: I think just practically.

MR. KROTIUK: It's a practical limit.

CHAIRMAN BANERJEE: So I go on loading this thing up with particles.

MR. KROTIUK: Right. Yes.

CHAIRMAN BANERJEE: And then when I increase the pressure on it, the bed doesn't compress any more.

MR. KROTIUK: Because that's what I was trying to show here, is that this is a situation where the particles and the fibers are at that upper limit, and if you add any more particles to this, you're going to get this situation, where you have a saturated - I'll just use that terminology - of particles and fibers, but then the rest of the particles, if it collects, is just going to be collected on top of the bed.

CHAIRMAN BANERJEE: All right. Let's look at what happens. Let's continue.

MR. KROTIUK: And then these two are homogeneous one-volume calculations, and the two-volume, and the heterogeneous are two-volume calculations, so I actually did pressure drops across two control loads. I mean, in actuality, you could use hundreds of control --

MR. WALLIS: Compress the fibers by the pressure drop through the top stuff.

MR. KROTIUK: Yes. This is the basic form in the --

CHAIRMAN BANERJEE: Your equations are very difficult to read. Maybe you should just use normal PowerPoint.

MR. KROTIUK: Oh, geez, it really did come out bad.

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: Yes, sorry about that. The Power Point --

CHAIRMAN BANERJEE: You should use black and white.

MR. KROTIUK: Yes, PowerPoint did - let me just see if it's bad. Well, let me just - consistent with the Ergun equation is a viscous term and a kinetic term. One of the things that I wanted to point out is that I derived this, and I didn't present the derivation here, but it's in the report - is that there's viscosity, velocity. This is that specific surface volume of the - whatever it is - fiber, particle. This is - X is a void ratio, and there's this one thing called K(X), which is the -- so there's a permeability relationship. And it basically --

CHAIRMAN BANERJEE: What is X?

MR. KROTIUK: Void ratio.

CHAIRMAN BANERJEE: Defined how?

MR. KROTIUK: My mind has just gone blank. Just give me one second.

CHAIRMAN BANERJEE: Epsilon is the porosity.

MR. KROTIUK: Epsilon is the porosity. This is the void --

CHAIRMAN BANERJEE: That's the void fraction.

MR. KROTIUK: It's a --

CHAIRMAN BANERJEE: It's a void to the --

MR. WALLIS: Why is there a viscosity in

the kinetic term? That's a correction factor that --

MR. KROTIUK: That's because -- okay, I'll get to that. Just give me a minute.

MR. WALLIS: A Reynolds number effect or something?

MR. KROTIUK: Yes, that's a Reynolds --

MR. WALLIS: Fairly weak effect, isn't it?

MR. KROTIUK: The whole kinetic term, as I indicated, is listed 8 percent of the total pressure drop.

MR. WALLIS: Even though the curves curve up?

MR. KROTIUK: Because this K(X) is non-linear. X is volume of the void over the volume of the solid, so it's Epsilon over one-minus Epsilon.

MR. WALLIS: So it's the fraction of the void filled by the particles. Is that what it is? Voids in the fibers. And X tells you something about how much - what's the volume of particles filling that void in-between the fibers?

MR. KROTIUK: That's the ratio of the volume of the void over the volume of the solid.

MR. WALLIS: Well, that's what I mean.

MR. KROTIUK: Okay.

MR. WALLIS: So it tells you how many

particles there are filling in the holes in the fiberglass.

MR. KROTIUK: Okay.

MR. WALLIS: How much --

CHAIRMAN BANERJEE: Depends how you are defining void here. Are you defining void as the fibers plus the particles, or --

MR. KROTIUK: No, void is void, nothing there.

CHAIRMAN BANERJEE: No, I mean when you're talking about --

MR. KROTIUK: When I talk about solid, it's the particles plus the fibers.

CHAIRMAN BANERJEE: So one minus Epsilon is particles plus fibers.

MR. WALLIS: One minus Epsilon - oh, that's the total solids?

CHAIRMAN BANERJEE: The total solids.

MR. KROTIUK: Total solids, right.

CHAIRMAN BANERJEE: Really, what you have are three parameters. In some sense, you have the volume fraction of the particles, the volume fraction of the fibers, and the volume fraction of the voidage.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: How would you put

Epsilon - one minus Epsilon being equal to the total solids.

MR. KROTIUK: You know, one thing I have to say is that I presented this equation in this form to try to show the similarity to the Ergun equation. I actually - and I didn't present this in the report - but this could actually be simplified to something that eliminates a lot of these duplications of the X, of the void ratios and the porosities, and all that. I just tried to present it in this form, because of familiarity.

CHAIRMAN BANERJEE: Okay.

MR. KROTIUK: Now to talk about this kinetic term, this term here with this exponent is really something that I came out - it's a semi-empirical impression that's used to -- it was derived, and I don't remember the reference, but it's for metal screens of weaves, and they specified whether the weave was cross, or this way, different type of weaves, but this is for something that related to any weave. And it also could be applied to beds composed of spherical particles.

MR. WALLIS: Some correlations don't have that factor in at all.

MR. KROTIUK: Right. But --

MR. WALLIS: Which case, the pressure drop becomes independent of the thickness of the bed, because it doesn't matter whether it's thick. It's the total amount of stuff there that gives you the pressure drop.

MR. KROTIUK: But, again, I didn't really investigate this too much, because when I started doing comparisons with test data, this term turned out to be so small. Typically, 8 percent was really an upper limit. It was more on the order of 1 to 2 percent of the total pressure drop, so I wasn't really too concerned with this term.

MR. WALLIS: Well, that can't give you the moving up of the pressure -- then you'll be linear, pressures will be linear with the velocity.

MR. KROTIUK: No, because of the K(X) factor, which is non-linear.

MR. WALLIS: Well, the K(X) has velocity in it?

MR. KROTIUK: You have --

CHAIRMAN BANERJEE: I thought K(X) only has the porosity in it.

MR. KROTIUK: Okay. I have --

CHAIRMAN BANERJEE: If you make K(X) a factor of velocity, then it's a non-linear term.

MR. WALLIS: I don't think it is, is it?

CHAIRMAN BANERJEE: No, I don't know his model yet.

MR. KROTIUK: Let me just -- all right, that's all. I wasn't really going to say more on the model, so let me just go to one of my -- okay. These are -- it's called a Happel Free Surface Model, and these are the relationships for K(X), so it's a function of void ratio.

MR. WALLIS: It's nothing to do with velocity. It's linear with velocity.

CHAIRMAN BANERJEE: As you would expect in creeping flow type of problems.

MR. WALLIS: What are we doing here?

MR. KROTIUK: Yes, that's something that you don't have. I just put it there because I thought maybe someone would ask. I'm going back.

MR. WALLIS: I think that thing about age, or about 20 significant figures.

MR. KROTIUK: That's a curve fit. We'll get to that.

MR. WALLIS: Okay. So the sum effect of permeability, which is messy.

MR. KROTIUK: And if I applied that relationship to a NUKON and cal sil mixture in the debris bed, basically I divided the total pressure drop into pressure drop due to the NUKON, pressure drop due to the cal sil. This is the viscous term and the kinetic term.

MR. WALLIS: This is something really new, of adding together these --

MR. KROTIUK: Yes. And then, of course, in addition there was exit and entrance effects.

MR. WALLIS: Which are small.

CHAIRMAN BANERJEE: So now that is more close to what we were saying, that basically you've superimposed these two.

MR. KROTIUK: That's right. It's the best thing I could come up with, that seemed to match.

MR. WALLIS: But there's no theoretical basis, is there, for adding together the two Kozeny terms like that?

MR. KROTIUK: It's a question of parallel --

MR. WALLIS: If you've got two things in series or something?

MR. KROTIUK: Parallel -- a series approach, I mean, you know.

CHAIRMAN BANERJEE: Well, these aren't series though. Right?

MR. KROTIUK: I mean, I tried both, to tell you the truth.

CHAIRMAN BANERJEE: They would be in series, according to this.

MR. KROTIUK: Right. This is --

MR. WALLIS: Not in series, they're actually mixed together.

MR. KROTIUK: I tried both approaches, and the parallel approach did not -- I couldn't get anywhere.

MR. WALLIS: The particles are inside the voids in the fibers, that's the problem.

CHAIRMAN BANERJEE: So what's the difference between this and having them in two layers then?

MR. KROTIUK: This equation --

CHAIRMAN BANERJEE: This is like having them in two layers.

MR. KROTIUK: This equation is applied to each layer.

CHAIRMAN BANERJEE: I know. But, I mean, if you look at this equation, it is like you're just having two layers, one of the cal sil, and one of --

MR. KRESS: They both have the same thickness.

MR. KROTIUK: They have the same thickness.

MR. KRESS: It's the thickness of the full bed. That's the difference.

CHAIRMAN BANERJEE: Yes. And this is the thickness of the full bed.

MR. KRESS: Yes. That makes it look strange to me, but if this is like a porous bed where the loop lets you do surface tension-like effects, then you could rationalize something like this.

CHAIRMAN BANERJEE: So let's say the basis for using this at the moment is empirical. Let's see where it leads.

MR. KRESS: Okay. Good approach.

MR. KROTIUK: Okay. Now talk about the compression and expansion. The references that I looked at seemed to have the best match-up by -- the best approach to the compression/expansion of the porous media was to assume that, as I said previously, that the first compression with a velocity increase was a non-recoverable, irreversible process, and then all the other compressions were elastic with constant compressibility.

MR. WALLIS: It doesn't recover when you decrease the velocity?

MR. KROTIUK: First --

CHAIRMAN BANERJEE: Not completely. Right?

MR. KROTIUK: Not completely. I mean, the references that I read, in actuality what happens is that you have the first one, first say compression will be some fraction, like 90 percent non-reversible, 10 percent reversible. Then it'll come back down, come back up, and then you may 60 percent non-recoverable, so there's no real cutoff. This is just a calculational approach, and from the references that I read, it seemed to indicate that this was a reasonable approach.

MR. WALLIS: Well, the first one is sort of a standard for in the filtration business.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: It's sort of like what --

MR. WALLIS: That's right.

MR. KROTIUK: Yes, I forgot the reference now.

MR. WALLIS: It's in my book, so it must be right.

MR. KROTIUK: Yes.

MR. KRESS: It must be out of date.

MR. KROTIUK: It's not entirely, because the difference is that this is X, not L.

MR. WALLIS: Well, that's strange. Did the particles effect the compressibility of the fiberglass?

MR. KROTIUK: Yes.

MR. WALLIS: If you take fiberglass and squeeze it mechanically, you can do this test. Mechanically, no fluid there at all.

MR. KROTIUK: Right. That's how they --

MR. WALLIS: Put particles in there, does it make a difference? I think it must eventually, because if you squeeze it too much, you're actually coming up against the particles.

MR. KROTIUK: The reference is from the AICHE Journal, and it's by Johnson & Johnson. It's actually a husband and a wife team.

MR. WALLIS: It goes back to --

MR. KROTIUK: They actually -- yes, right. They reference it, but they actually did those experiments where they compressed.

MR. WALLIS: Right.

MR. KROTIUK: And they said that this looks like a reasonable approach.

CHAIRMAN BANERJEE: So, I mean, phrasing

it in terms of X, rather than L, simply changes N. Right?

MR. WALLIS: N is always about one, two, three, six.

MR. KROTIUK: It's the same number that you came up with, about that order of magnitude.

MR. ABDEL-KHALIK: Can we go back to the previous slide?

MR. KROTIUK: Sure.

MR. ABDEL-KHALIK: How are these individual porosities defined? Epsilon cal sil, for example, what is the definition of that?

MR. KROTIUK: Cal sil is only considering the presence of cal sil --

MR. ABDEL-KHALIK: The other material doesn't exist.

MR. KROTIUK: Does not exist, right.

CHAIRMAN BANERJEE: Is the volume of cal sil over the total volume?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: If I understand what you're doing.

MR. KROTIUK: Volume of cal sil over the total.

MR. WALLIS: But it's not layered, because if they were in series there would be different volumes.

CHAIRMAN BANERJEE: One minus. At least that's how I understand your --

MR. WALLIS: It's somewhat strange.

MR. KROTIUK: It is. And, I agree, I was trying different things, and basically trying to use parallel and series approaches.

MR. WALLIS: It's a bold step forward, which works fine if it's only cal sil, and only --

MR. KROTIUK: Yes. Right.

CHAIRMAN BANERJEE: You know, what I said earlier was let's assume this on an empirical basis for now and see where it leads. Clearly, the equation has almost no justification.

MR. WALLIS: My feeling about this was it was very promising, but it really needed a much broader experimental base in order to verify the equation.

MR. KROTIUK: I used experiments that -- actually, in the NUREG, I compare this to not only the PNNL data, but to the ANL data, and to LANL data, so there's a lot of comparisons.

MR. WALLIS: Okay.

MR. KROTIUK: I'm only presenting a

limited number of cases here.

MR. WALLIS: This compression equation is very interesting, because if P is zero, then X is infinite.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: The problem, of course, that you face is that if you look at your Ergun equation, it doesn't care how the porosity is distributed. And when you put your fibers and your particles in there, you're effecting the tocquicity, or whatever the hell it's called. And that's what affects the pressure drop. I mean the fluid dynamics of it is just getting different sort of flow path than you --

MR. KROTIUK: Yes, in the paper by Johnson & Johnson, what they actually did is that they used an equation similar to this, but they actually divided up their debris bed into a fairly large number of control volumes. They would go - not just two - I forget, 10 or 20, or something like that, so that they could predict the pressure drop distribution within the bed, because it's non-linear, as I said earlier. And I just thought that for a practical application, it was not reasonable to do that, so I was trying to come up with a hand calculation that would give me upper and lower limits.

MR. WALLIS: That's what Ingmanson does. The bottom of the bed is compressed much more than the top.

MR. KROTIUK: Right.

MR. WALLIS: And so you get a different pressure going into the bottom than the top.

MR. KROTIUK: Yes, you get a whole distribution.

MR. WALLIS: And you can get some solutions- if it's linear like this, you can get some form solutions, as long as Epsilon isn't too big.

MR. KROTIUK: Yes. It was an approximate approach that I was trying to --

MR. ABDEL-KHALIK: Going back to the definition of these Epsilons.

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: So let's say Epsilon cal sil, that is as if you've taken the NUKON and replaced it with liquid.

MR. KROTIUK: Right. That's correct.

MR. ABDEL-KHALIK: You have a composition, so that's physically what this picture looks like.

MR. KROTIUK: That's correct.

CHAIRMAN BANERJEE: You have to think

about this.

MR. ABDEL-KHALIK: Right.

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: Because you really can combine all the Epsilon and X terms, and come up with just one function in terms of Epsilon. It turns out to be one minus Epsilon cubed divided by Epsilon, a product of the two X cubed times one minus Epsilon squared divided by one plus X squared times Epsilon cubed, that's just one minus Epsilon cubed --

MR. WALLIS: I think you have a problem if there's no cal sil there at all, because then Epsilon is zero.

MR. KROTIUK: That's right. There is a problem, yes.

MR. WALLIS: It doesn't make any sense.

MR. KROTIUK: No. And one of the things when I was looking at this equation, I was trying to look at the extremes of all NUKON and all cal sil.

MR. WALLIS: Maybe X cal sil cancels Epsilon cal sil, or something. There has to be something there.

MR. KROTIUK: Again, in the -- I recognize that. Okay? I recognize that there's a problem at one of the upper limits, and in --

MR. WALLIS: The real problem, you have nothing there has an infinite effect.

MR. KROTIUK: And there are -- what happens -- there's a couple of things that happen. One is that in the actual application that's outlined in the NUREG, there are upper limits extremes, but what happens also is that the K(X) value also feeds into it, and the K(X) value becomes very large at one point, so that you're dividing by a large number, and it becomes --

MR. WALLIS: Zero over zero.

MR. KROTIUK: Yes, so you have to -- I tried to specify it very much in the application section of the NUREG to say what happens when you approach upper limits, and what to use at those upper limit values.

CHAIRMAN BANERJEE: So what is the second equation? The first one we have some understanding of.

MR. KROTIUK: This equation came from, as I said, Johnson & Johnson in an AICHE paper, in an AICHE Journal, and they derived this equation in there, and I used it just the way they derived it.

CHAIRMAN BANERJEE: So this is to account for the sort of non-linear --

MR. KROTIUK: This is to account for the elastic range.

MR. WALLIS: I'm tempted to say that Johnson & Johnson manufacture band-aids.

(Laughter.)

CHAIRMAN BANERJEE: So they got this by dividing the layer into many thin bits, and integrating with something.

MR. WALLIS: Anyway, we can move on, I think. There's some basis for this one.

MR. KROTIUK: You want me to --

CHAIRMAN BANERJEE: Well, where are we going now?

MR. KROTIUK: Well, I was just going to --

MR. WALLIS: Break, aren't we?

CHAIRMAN BANERJEE: Yes, but what is the next few slides about?

MR. KROTIUK: The ones are just coming up with the various --

CHAIRMAN BANERJEE: That is your equation.

MR. KROTIUK: That's basically the equation. And, as I said, there's a lot of derivation in the paper. I didn't want to present the whole derivation. It goes on for about 20 pages.

CHAIRMAN BANERJEE: And you're going to apply this to --

MR. WALLIS: A whole lot of stuff.

CHAIRMAN BANERJEE: -- the two separated layers, and all this sort of stuff.

MR. WALLIS: He's going to show us it's better than CR6224.

CHAIRMAN BANERJEE: And show us how it agrees.

MR. KROTIUK: I'm sorry?

CHAIRMAN BANERJEE: How it agrees with the data.

MR. KROTIUK: How it agrees, or doesn't agree.

CHAIRMAN BANERJEE: Doesn't agree. All right. So this is a good time to take a break then. Okay. So we take a break for 10 minutes, so back at 20 to 4.

(Whereupon, the proceedings went off the record at 3:32:37 p.m., and went back on the record at 3:46:08 p.m.)

CHAIRMAN BANERJEE: Okay. Back in session. So perhaps we could try to finish up about 4:30.

MR. KROTIUK: I think that's possible.

CHAIRMAN BANERJEE: All right.

MR. KROTIUK: As I said before the break, what I want to do now is just indicate some of the values that are in equation and what I determine the value should be. One is that from the test data, one of the things that you needed to know was the specific surface area of the NUKON and cal sil fibers. And, also, for any fibers in the cal sil that's included in the cal sil. And looking at the test data, and seeing what fit and everything else, and pluses or minuses, I did a fairly detailed assessment of that, and these are the numbers that I came up with.

For comparison, just so that you know, this 300,000 number for the NUKON, what 6224 recommends is a number around 171,000. However, the cal sil number of 650,000 is about the right order of magnitude, which the 6224 correlation uses, which is 600,000. The densities for the NUKON and cal sil fibers, they agreed with what was previously presented in the various NUREGs, and what PNNL did is they actually did measure the densities of the fibers, and the particles, so they agreed.

One of the things that has to be -- sorry. One of the things that has to be known to apply the methodology I've developed is the initial thickness of the debris bed, so what I did is that since we formed the debris bed at .1 foot per second which I termed as a relatively low velocity, it's not super low, but relatively low, I looked at all the test data, and derived this relationship for the initial total bed thickness relating the void ratio of the NUKON, the area, masses of the NUKON mixed with cal sil, the densities indicated up here. And the question is, is what value for the void ratio of NUKON and cal sil would match the test data? And, basically, looking at the test data and backfitting it, these are the numbers that I came up with. And let me just show you the plot.

For instance, on this plot here, the circles are the calculation. The solid diamonds in blue, and the pink also are the measurements of the actual bed thickness at .1 foot per second. The only one that differs is this one happens to be at .2 feet per second.

MR. WALLIS: Now if it were at a different velocity what would be the difference?

MR. KROTIUK: It will be different.

MR. WALLIS: Well, that depends on the pressure drop, doesn't it? If you're worried about compression, the pressure drop has got to come into it, too, not just velocity.

MR. KROTIUK: Well, if you look at the relationship, the only thing I have in here is the masses, the densities, and the area and these empirical value for the void ratio. I actually derived this. This is derived from --

MR. WALLIS: Assuming no compression. Is that what it is?

MR. KROTIUK: Assumes no compression, yes.

MR. WALLIS: So your compression theory has a pressure in there.

MR. KROTIUK: Yes.

MR. WALLIS: At the initial thickness, but there's no pressure here.

MR. KROTIUK: No, there is a pressure - I'm sorry. There is a pressure at this thickness, and that's--

MR. WALLIS: You have to calculate that?

MR. KROTIUK: I mean, there is -- excuse me?

MR. WALLIS: You have to calculate that from your theory or something?

MR. KROTIUK: There is an iteration process in the calculation whereby you would calculate this PM prime, which is the starting point. But to do that, you need the thickness. As I said, I don't want to go through the whole application methodology, but there is an iteration process. You have to know the initial thickness, and then there's iterative process to calculate the PM prime at that thickness.

MR. ABDEL-KHALIK: I mean, going back to the question I asked earlier about how thick those cal sil beds. If I use your density, and for a .45 kilograms per meter squared loading, I end up - if I were to assume that I have a solid layer of cal sil with that loading, it would end up being .25 millimeters thick.

MR. KROTIUK: This material density is the density of the material.

MR. ABDEL-KHALIK: Right, density of the solid. Right.

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: Now when you say that the actual height of the bed --

MR. KROTIUK: Is larger.

MR. ABDEL-KHALIK: -- is six millimeters, roughly a quarter of an inch, that means this is a very, very highly voided layer.

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: Right?

MR. KROTIUK: Yes.

MR. WALLIS: Just cal sil?

MR. ABDEL-KHALIK: Right. Which, to me, is very surprising, given the fact that cal sil is very, very small particles.

MR. KROTIUK: And the only thing, as I said, I did a lot of -- I looked at a lot of the data at the .1 feet per second for NUKON and NUKON cal sil, and I had no data for cal sil alone, so I back -- I could come up with this parameter here for the NUKON because I had NUKON only data, but this one had to be backed out of basically the NUKON cal sil data, this value here.

MR. ABDEL-KHALIK: But if I were to just use his observation that the thickness of this layer in that case was roughly a quarter of an inch, I would get a ratio about 25 for cal sil only.

MR. KROTIUK: Well, I have to admit that that was - the value for the cal sil was a weakness in my derivation, and if I had cal sil data, I could have come up with something. This was the best thing I could come up.

MR. MICHENER: Tom Michener from PNNL. Also, it wasn't really what we called a formed bed either with the holes in it.

MR. KROTIUK: Yes, that was --

MR. ABDEL-KHALIK: Looking at the pictures, there are only a few holes. When you say --

MR. MICHENER: There was quite a few holes.

MR. KROTIUK: Quite a lot of holes.

MR. MICHENER: Quite a few holes, and it just depended on when you stopped, too. Because, like I said, it just kept bursting through. But yes, we never did what we call form an official cal sil only bed, as I recall.

MR. KROTIUK: No. Closest we came was in a bench top, which was better than the one in the large loop, but it still had holes in it.

MR. ABDEL-KHALIK: The point I was trying to make is that all you need to come up with a value for X for cal sil is to come up with a thickness, an actual measured thickness, or average thickness in a loading and that gives you a value of X.

CHAIRMAN BANERJEE: You don't even have to have a sieve, you just have to load it.

MR. ABDEL-KHALIK: Right.

MR. WALLIS: This curve here with this figure here is thickness versus amount of stuff.

MR. KROTIUK: Yes.

MR. WALLIS: It doesn't say anything about composition, really.

MR. KROTIUK: This is a total.

MR. WALLIS: Total amount of stuff.

MR. KROTIUK: This is NUKON and cal sil.

MR. WALLIS: Oh, it doesn't matter what it's made out of?

MR. KROTIUK: That's what I plotted it as, but if you look at the equation, the equation does matter, because you have -- the NUKON mass and the cal sil mass is separate, but just for this plot --

MR. WALLIS: The circles are just sort of a straight line through the middle of the picture.

MR. KROTIUK: It's not quite, because if you notice, there are dips in it. It's not quite a straight line.

MR. WALLIS: But it really seems to indicate if the circles are right, that you're predicting that the thickness simply depends on the weight of stuff. It's dependent on what it's made out of, cal sil or --

MR. KROTIUK: Well, if you look at the equation here is that --

MR. WALLIS: I'm just looking at the

circles.

MR. KROTIUK: I know, but if you look at -- NUKON has a greater effect than the cal sil does, from what I --

MR. WALLIS: Because of X.

MR. KROTIUK: So if you plot this same curve which I've done, saying the NUKON, you also get a NUKON concentration so that the total concentration --

MR. WALLIS: Isn't the thickness really determined by the NUKON, essentially? Cal sil just fills in the voids in the NUKON. It doesn't make any difference.

MR. KROTIUK: Yes. And that's the reason why this is 6 and that's 30.

MR. WALLIS: I would think that it's even more so than that, because the cal sil can fit in-between the NUKON fibers, that it's really the NUKON that decides how thick the bed is. Anyway, let's go on.

MR. KROTIUK: Yes. I feel more comfortable with the 30 number than with the 6.2.

MR. WALLIS: Okay. The NUKON dominates anyway.

MR. KROTIUK: The NUKON dominates. Now

this is the real -- this is where I was -- I went out most on the limb.

MR. WALLIS: I see.

MR. KROTIUK: Yes.

MR. WALLIS: I like the degree of accuracy.

MR. KROTIUK: This is completely a curve fit. There is no theory to this. I had -- basically, this is the relationship for that minimum, what I call a saturation thickness over the total initial thickness as a function of the mass of cal sil and NUKON. And I plotted up this data --

MR. WALLIS: Exponential?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: As you see in the next slide.

MR. KROTIUK: And I emphasize here that this is -- it could definitely be improved, but I -- this is the best I could come up with. I tried --

MR. WALLIS: Is this for saturation?

MR. KROTIUK: Yes.

MR. WALLIS: So when you've got a very small amount of cal sil, a tiny amount of it can saturate?

MR. KROTIUK: Yes, it can.

MR. WALLIS: Doesn't make sense.

MR. KROTIUK: But it can. You could get a saturation layer on top in the fiber bed, if that -- but the pressure drop that you would have for that layer will really be calculated by the pressure drop equation.

MR. WALLIS: It's because it's not homogeneous.

MR. KROTIUK: It's not homogeneous.

MR. WALLIS: Well, okay. That's the case, but if it's homogeneous and filling in the pores, then it wouldn't be an exponential relationship.

MR. KROTIUK: Right.

MR. WALLIS: It would presumably just be simply a certain mass ratio.

MR. ABDEL-KHALIK: I mean, your exponential fit is driven by that one outlier data point, that one.

MR. KROTIUK: That's right.

MR. WALLIS: Right.

CHAIRMAN BANERJEE: How secure is that point?

MR. KROTIUK: Well, this is one of those points that after I got all the data together, and we looked at testing that we completed, it was saying geez, I wish I had more points out here.

MR. WALLIS: Let's get this clear. I don't understand this at all. I thought that this saturation was when the particles of cal sil essentially filled the voids in the fiber.

MR. KROTIUK: That's correct.

MR. WALLIS: Which would simply be a mass ratio, it doesn't matter how much -- there's no --

MR. KROTIUK: Remember --

MR. WALLIS: You have twice as much fiberglass, you have twice as much cal sil, so --

MR. KROTIUK: Remember what I said, that this is a practical limit. I'm trying to use test data to come up with a practical limit. This is not a theoretical upper limit, it's a practical limit.

MR. WALLIS: And I think your practical limit must be including the distribution throughout the bed or something, because it doesn't make sense.

MR. KROTIUK: No. Okay. What I -- okay, let me explain to you how I did this. Okay? What I did is that I took my two-volume model, looked at test data. I had pressure versus velocity for NUKON cal sil beds. I assumed a thickness for the saturated control volume, and then assumed that the second control volume was entirely fiber, and I did the pressure drop calculations for varying thickness of this saturated layer. And when I calculated a thickness that matched the test data, and it matched up fairly well. I included the plots in the NUREG, that are these points that I plotted here.

MR. WALLIS: So it's very dependent upon the model you're assuming. There's no direct measurement.

MR. KROTIUK: There's no direct measurement of this, no. I couldn't make direct measurements. As I said earlier, if we made direct measurements, then we wouldn't have cal sil masses.

MR. ABDEL-KHALIK: Now let's look at the limiting case, where you have zero of cal sil. This equation would predict a ratio between delta L-min and delta L-initial of .007 something. Right?

MR. KROTIUK: That's if you --

MR. ABDEL-KHALIK: But shouldn't this value, this number in front of the exponent be just simply one divided by X for NUKON?

MR. KROTIUK: That's a good -- I didn't think about that. Yes.

MR. ABDEL-KHALIK: I mean, isn't that physically what's going on?

MR. KROTIUK: I have to think about that.

MR. WALLIS: Particles are filling the voids.

MR. ABDEL-KHALIK: I mean, you don't have any more cal sil in. Right? And what you're doing is compressing the NUKON by itself. And if that is the case, you're essentially turning it into a solid layer. And if that's the case, the ratio is just one over X.

CHAIRMAN BANERJEE: And the other extreme, when you form only a cal sil bed, of course, you get a weird answer.

MR. KROTIUK: Yes, you get a weird --

MR. WALLIS: You have this NUKON.

MR. KROTIUK: As I said, this I considered the -- this is the hardest part of this derivation, because all the rest of the parts, I had some theoretical basis for it, maybe there was a little empiricism in it or something. This one was completely empirical --

MR. ABDEL-KHALIK: You have your metric limits that you can apply.

MR. WALLIS: So can you tell me more what this delta L-min, and delta L-initial mean here?

MR. KROTIUK: Okay. Let me go --

MR. WALLIS: I missed something.

MR. KROTIUK: Right. Let me go back here.

MR. WALLIS: What's delta L?

MR. KROTIUK: My delta L-min is this - I'm sorry - let me give it to you. It's this thickness right here. The delta L is the entire thickness.

MR. WALLIS: So I still don't understand at all now. You're saying that you've got a little layer of cal sil on top of a lot of fibers when this ratio is .001?

MR. KROTIUK: That's correct.

MR. WALLIS: But it doesn't say anything about how you saturate the fiber bed.

MR. KROTIUK: Okay. Because I'm not trying to calculate how I saturate the fiber bed. I'm saying I want to calculate a practical upper limit for pressure drop.

MR. WALLIS: Well, that's all together different. So you're saying what's the thickness of this thin layer on top of the fiber layer?

MR. KROTIUK: That's right.

MR. WALLIS: It has nothing to do with --

CHAIRMAN BANERJEE: But it's more to - the last one on the right seems to be --

MR. KROTIUK: This is a case that we actually didn't test. This is a case where you have so much particles in the fiber that the particles start collecting above the fiber bed.

CHAIRMAN BANERJEE: Isn't that more or less like what you showed us in those micrographs?

MR. KROTIUK: No, because in those SEMs, there were a mixture of cal sil and fibers, particles and fibers.

CHAIRMAN BANERJEE: It looked almost like you had a layer of cal sil, and then there was a region of fibers plus cal sil.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: Rather than --

MR. KROTIUK: But it --

CHAIRMAN BANERJEE: It was more like on the right, than that one. At least, if you go back to the micrographs, so it looked like this was primarily cal sil, and then on the left you had a mixture of cal sil and fibers.

MR. KROTIUK: Well, one thing you could say is this is primarily fibers, the amount of cal sil is really small.

MR. WALLIS: Right.

MR. KROTIUK: And I could see fibers in here, but you see the volumes. It's by volume, it was 59 percent. This was measured, 59 percent cal sil, 6.5 percent NUKON.

MR. WALLIS: Was delta L-min the thickness of that layer on the right?

MR. KROTIUK: I'm sorry, say again?

MR. WALLIS: This delta L-min, is that --

MR. KROTIUK: This is this layer.

MR. WALLIS: So you're not just talking about the thickness ratio of the various layers?

MR. KROTIUK: Yes, well, it's - the thickness ratio that I'm calculating is this over the entire bed thickness.

MR. WALLIS: You're already assuming two layers.

MR. KROTIUK: I'm assuming two layers, right. I'm saying to do this calculation, I have to have -- I'm assuming two layers.

MR. ABDEL-KHALIK: But like Dr. Banerjee suggested, this corresponds to the last image on the right, rather than the third image on the right.

CHAIRMAN BANERJEE: It's closer, anyway.

MR. ABDEL-KHALIK: Right. I mean, this is like, you know, you have particles inside the voids between the fibers.

MR. KROTIUK: Right.

MR. ABDEL-KHALIK: And then you squeeze on it, the particles come out because you can't keep too many of them inside the fibers.

MR. KROTIUK: Right.

MR. ABDEL-KHALIK: And that's why you have a layer of particles on top.

MR. WALLIS: Let's just follow from geometry. If you put all the cal sil on top of the fibers, you can figure out how thick the layer is. You don't need any correlation at all.

MR. KROTIUK: You know, that's a valid point.

CHAIRMAN BANERJEE: In one case you have cal sil amongst the fibers, in the other case you have fibers amongst the cal sil, more or less.

MR. KROTIUK: Yes. And I'm assuming for that, that's it all fiber.

CHAIRMAN BANERJEE: Yes. So if you look on the left, your volume fraction of the cal sil to the NUKON is a factor --

MR. WALLIS: I think there's something sort of circular here, is you have this thing which is somewhat vague, this delta L-min, which you derived from the test data, and then you used your correlation to predict the test data, so this is a kind of mammoth fudge factor, this exponential thing.

MR. KROTIUK: In actuality it isn't, because if I -- when I show you the results, it just doesn't automatically calculate.

MR. WALLIS: Oh, okay.

MR. KROTIUK: But I like the comment that was made, that to relate this -- I had a heck of a time to try to come up with that, the value for the delta L-min for this thickness here. And maybe I was thinking too much, and I didn't think geometry. I should have thought of that.

MR. WALLIS: So you say if there is a thin bed, delta L-min is its thickness?

MR. KROTIUK: Correct.

CHAIRMAN BANERJEE: Go back to that, again. There's something very weird about that, because if you take the mass of cal sil over the mass of NUKON, in that layer you showed us, which was 60 percent cal sil, and a little bit of NUKON, that's a very large number.

MR. KROTIUK: That's by volume.

CHAIRMAN BANERJEE: Yes, but the volume and mass are related by the density, which is still a very large number.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: So that becomes a huge number, it's also exponential. I just don't see that it --

MR. WALLIS: It's a very strange --

MR. KROTIUK: And the thing is, I agree with you, but it's --

CHAIRMAN BANERJEE: That ratio is in the exponential?

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: Wow.

MR. WALLIS: No, it's the total mass, isn't it? The total mass in the bed is M cal sil over M NUKON, total mass in the bed, in the whole thing.

MR. KROTIUK: The total mass in the bed of M cal sil --

CHAIRMAN BANERJEE: Oh, not in that layer.

MR. KROTIUK: Not in the layer. I'm sorry.

MR. WALLIS: But this must be just geometry, if your total mass in there, and you put it on the surface, you know how much is there, too.

MR. KROTIUK: And I'm agreeing with you, but at the time I was doing this, I was really thinking of how to handle this, and that just didn't dawn on me.

CHAIRMAN BANERJEE: Well, I think you

should take another look at this equation.

MR. KROTIUK: I put on the bottom here that I -- this was my indication that I'm not completely satisfied with this equation, but it's what I could come up with.

CHAIRMAN BANERJEE: Well, the simplest model is to have two separated layers. Right?

MR. KROTIUK: And I have two separate layers.

MR. WALLIS: One is all NUKON, and one is all cal sil. Just put the cal sil bed on top of the NUKON bed.

CHAIRMAN BANERJEE: What difference does it make?

MR. KROTIUK: That's an interesting approach.

MR. WALLIS: Isn't that what you did?

CHAIRMAN BANERJEE: No, he didn't.

MR. KROTIUK: No, I didn't, I mixed it.

MR. WALLIS: One possible case is presumably a cal sil bed on top of a NUKON bed. Then you just have them in series, and then simple --

MR. KROTIUK: Then you have to --

CHAIRMAN BANERJEE: The series works?

MR. KROTIUK: Yes, but then you still have to have a way of calculating the thickness of the cal sil portion.

CHAIRMAN BANERJEE: It's all cal sil.

MR. KROTIUK: Yes.

MR. WALLIS: It's all cal sil, it's all up there.

MR. KROTIUK: Yes, but what thickness are you going to assign to it?

MR. WALLIS: Isn't that effect obtainable with all cal sil, or do you have to have the cal sil within the NUKON matrix to get a thin bed effect? I don't know. It's never been clear to me what a thin bed effect is.

MR. KROTIUK: It was never clear to me. I just --

CHAIRMAN BANERJEE: But first - I mean, you are just trying to bound it. Right?

MR. KROTIUK: I'm trying to bound it.

CHAIRMAN BANERJEE: The lower bound is like, it seems to me that if you completely separated the bed and put all your cal sil in the impervious layer, that's really the worst condition.

MR. KROTIUK: Yes. The problem that I see, the practical problem, as I said, is to calculate the thickness of that cal sil layer.

MR. WALLIS: But you know how much cal sil there is.

CHAIRMAN BANERJEE: You know that.

MR. KROTIUK: You know how much cal sil that is, but you don't know the --

MR. WALLIS: The density of it?

MR. KROTIUK: You know the density of the --

MR. WALLIS: You don't know it's Epsilon?

MR. KROTIUK: No, you don't know its Epsilon. Right.

CHAIRMAN BANERJEE: But it seems easier to estimate than this very --

MR. WALLIS: Take some cal sil and measure it. I think it's --

CHAIRMAN BANERJEE: Yes, spray it on something and see --

MR. WALLIS: I think it's fairly high. I mean, it's 40 percent solid, or something.

CHAIRMAN BANERJEE: It's going to be --

MR. WALLIS: It's fairly dense stuff.

CHAIRMAN BANERJEE: I think that's what Said was getting at when he said how thick was that bed that you found?

MR. ABDEL-KHALIK: Yes.

MR. KROTIUK: Well, in all honesty, just I didn't do that calculation, so I --

CHAIRMAN BANERJEE: A few holes in it.

MR. KROTIUK: -- don't know what you would come up with, but I'm wondering that if you did do that calculation whether you would be coming up with such a high pressure drop, that maybe it's not practical.

MR. WALLIS: Well, I think it's not quite as simple as that. I mean, you have this bed that's being built up. What happens is you get maybe the coarse particles first, and then you get finer particles, and then the finer particles fill the holes in that, and the finer finer particles fill the holes, until eventually you apply everything. This is very different from just shaking it up and measuring the void fraction. There's a dynamic flow effect, which is driving particles into the holes until they block them, making a kind of check valve. There's a plugging effect because of the flow, which wouldn't occur if you just measure the void fraction.

CHAIRMAN BANERJEE: Well, but that's -- unfortunately, you couldn't get a pure cal sil bed, so you got holes in it. But if you could have got a pure cal sil bed, you have a measure of it.

MR. WALLIS: That depends on how it's formed.

CHAIRMAN BANERJEE: It matters, of course. Though, if -- yes, if the fine particles pass through it, eventually you --

MR. WALLIS: Well, think of it. I mean, you have some -- a bed of rocks, and gravel, and clay, and so on. Eventually, the clay fills in the holes.

MR. KROTIUK: I think the bottom line of all this discussion is that this is something that I recognize needs further thought, but this is the best I could come with.

MR. WALLIS: I think we're very suspicious of this.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: It should at least go to the right limits.

MR. GEIGER: I hear you, and --

MR. WALLIS: Especially that last digit, too. I mean --

(Laughter.)

MR. KROTIUK: It's a curve fit. I don't have any more to say about it, but that.

MR. KRESS: It seems to me like you ought to get two linear curves out of that. As you're filling in the fibers, you get one linear curve, and then you completely fill in the fibers with the cal sil, and you're just now putting it on the top, and you ought to get another layer curve, so this looks like to me the curve fit you had ought really be two straight lines.

MR. ABDEL-KHALIK: But he's hanging his entire hat on just one data point.

MR. KRESS: Yes, well that data point is - one of these endpoints, maybe one of these endpoints he's talking about, where you need to approach a lot of endpoints.

MR. KROTIUK: I have no comment on that, except that that's data that I had.

CHAIRMAN BANERJEE: Well, let's move on and see where it gets to.

MR. KROTIUK: Okay. Now just some comparisons between test data, and calculations. These are the NUKON only tests, so I'll concentrate in this area. And, basically, I'm going to show two comparisons, this one, and this one in blue and italics. And this curve, what I tried to do is to come up with some sort of numerical quantifiable number to say how good the correlation was with regard to the test data. So I defined this parameter, delta

P prediction of minus delta P test data over delta P test data. And I averaged it for all of the points, and then I listed here a maximum and a minimum, so this is indicative of how good the correlation was.

MR. WALLIS: There are no fudge factors in here?

MR. KROTIUK: None whatsoever.

MR. WALLIS: Does the compression make a difference? You have to have an N or something. Does the compressibility make a difference?

MR. KROTIUK: That was in the 236. I didn't present that, but I used the test data, which I had test data that was basically measured thickness versus pressure, and I used that to come up with --

MR. WALLIS: Used the measured thickness.

MR. KROTIUK: The measured thickness.

MR. WALLIS: Doesn't that make a difference, is the pressure drop considerably higher if you consider the compression or not? Does it make much -- does it have much effect?

MR. KROTIUK: But if I use the --

MR. WALLIS: If you just ignore compression, does it matter?

MR. KROTIUK: For the 236, for the value of N?

MR. WALLIS: No, if you simply -- you have a theory which says the bed compresses as the pressure drop goes up, and this increases the pressure drop.

MR. KROTIUK: Right.

MR. WALLIS: You ignore that effect completely and say the bed doesn't compress at all, are you way off? Do you really have to consider that?

MR. KROTIUK: Yes, you do have to consider it.

MR. WALLIS: And that's a significant -- so you have to get that right.

MR. KROTIUK: I wills how a comparison on that.

MR. WALLIS: Okay.

MR. KROTIUK: I'm going to show a comparison. I actually have a comparison of that. Okay. So, as I said, this is not --

MR. WALLIS: Because that introduces a non-linearity into the thing.

MR. KROTIUK: Right.

MR. WALLIS: As the pressure drop goes up, the voids change, and so --

MR. KROTIUK: So, as I said, this is not a ideal comparison, but it is a comparison to give you an idea of how good it was. So let's look at the first comparison, which is this one here. And this was the NUKON bed, and it had .58 kilograms per meter squared of NUKON, is that around 20 degrees C. This is the test data, this is the one volume calculation. And these are the value that I used, the 300,000 foot to the minus one. Just for comparison, if I used the NUREG/CR-6224 correlation, it came - this was the result of the calculation.

MR. WALLIS: And if you use the NUREG S value in your correlation --

MR. KROTIUK: Say again?

MR. WALLIS: You're using the NUREG correlation here.

MR. KROTIUK: The NUREG correlation.

MR. WALLIS: And you're using your correlation with your S value.

MR. KROTIUK: Right. This is the -- my correlation, the one-volume model with 300,000.

MR. WALLIS: It looks to me if you use 171,000 in your correlation, you might come out about right.

CHAIRMAN BANERJEE: Or you may come down to the NUREG.

MR. KROTIUK: They come down, but remember, I -- if you remember here, I have a fair number of NUKON test data points, and I could use that using the thickness as a function of velocity, and delta P and all that, you could actually calculate of S to be as specific surface area. And when I looked at that, I was trying to do two things, get a number that was matched to data fairly one, but also try to give me a number that was at the upper limit to give a conservative calculation. So you're right, in this particular case, if I had like maybe that 171, I would maybe match the data, but I'm saying, I'm using 300K because that's what I want to use that will give me a conservative value for all the data that has been collected.

MR. ABDEL-KHALIK: Now which test series does this point follow?

MR. KROTIUK: This is from test series 2.

MR. ABDEL-KHALIK: So it is not in this table, in the table on the previous - or is it somewhere in this table on page 42?

MR. KROTIUK: It should be there. Let's see, 63313, it's right here.

MR. ABDEL-KHALIK: Which one?

MR. KROTIUK: This one right here.

MR. ABDEL-KHALIK: This one.

MR. KROTIUK: It's just that it's in

italics.

MR. ABDEL-KHALIK: Okay. Now if you look at this table, this entire series, this definition of the ratio, the maximum value of the deviation, fractional deviation was 1.88, and the minimum value of the deviation was .664.

MR. KROTIUK: Right.

MR. ABDEL-KHALIK: Which means I guess the predicted value should be off from the measurement at least by 66 percent.

MR. KROTIUK: Yes, I --

MR. ABDEL-KHALIK: According to this definition.

MR. KROTIUK: At a given data point, yes.

MR. ABDEL-KHALIK: Right. But that's not what the graph shows. This is not a 66 percent difference between data and prediction.

MR. WALLIS: It's fairly big though, isn't it? When you say 66 percent --

CHAIRMAN BANERJEE: It's a log scale.

MR. ABDEL-KHALIK: No, it is not a log scale.

MR. KROTIUK: That's not a log scale.

CHAIRMAN BANERJEE: Oh, it's not?

MR. WALLIS: When you say 66 percent, how do you define that?

MR. ABDEL-KHALIK: Delta P.

MR. KROTIUK: The definition is up on the top here.

MR. WALLIS: Over delta P data, or delta P over delta P theory?

MR. KROTIUK: It's delta P prediction minus delta P data over delta --

MR. ABDEL-KHALIK: Error divided by the measurement.

MR. KROTIUK: Error divided --

CHAIRMAN BANERJEE: That's closer to 100 percent.

MR. WALLIS: It's close to 100 percent.

MR. ABDEL-KHALIK: Right. So, I mean, I would have expected the prediction to be off from the data by nearly a factor of 2, based on the values that you've given in this table. But that's not the case here.

MR. KROTIUK: Well, you have -- over here it is. You have about a 2 from the data, and this is --

MR. ABDEL-KHALIK: Oh, I'm sorry. I misread this table. I misread this chart. Thank you. Thank you. Sorry.

MR. KROTIUK: Now let me go to the next one, because this is the prediction and the data for the bed thickness. Again, the data is in black, the predicted bed thickness that I predicted is in this red color.

MR. WALLIS: The data are the diamonds?

MR. KROTIUK: The data are the diamonds, right. And I just tried to differentiate between the first compression, and then the subsequent compressions. So the key thing that I'm trying to show here is that there is a variation in the bed thickness as a function of velocity.

MR. WALLIS: What you should really do is plot the bed thickness versus this pressure drop, because that's part of the load on it, which is what the theory says it is.

MR. KROTIUK: You know, velocity is related to pressure drop, you know.

MR. WALLIS: Yes, but the equation you use is the delta P to the N or something, and if you actually plotted that, you could show the equation itself has the right form.

MR. KROTIUK: Yes, okay. Let's see, did I do that?

MR. WALLIS: Very sparse data, you really need --

MR. KROTIUK: Well, this is only one test. I mean, we had data for every single one of those tests. Let's go to the next one.

MR. WALLIS: What do we conclude from this?

MR. KROTIUK: Well, let me go to the next before making a conclusion. This is the other NUKON test, NUKON only test, and in this case, the test data is in black. This is the one-volume model prediction. And it's very close to the test data. And this is the reason why I said I recommended using the 300,000 value, because this was one of the cases that told me to use it, because the other one really, as you said, should have a lower value, but I wanted to bound all the data. Okay? And this is now the comparison of the one-volume model thicknesses versus velocity, and the data.

CHAIRMAN BANERJEE: Now here the thickness is predicted to be more than the data.

MR. KROTIUK: Well, initially it's close, but then there's differences here. Considering everything, I think it's pretty close.

CHAIRMAN BANERJEE: It's not a bound that you're doing.

MR. KROTIUK: For the thickness, no. I'm trying to bound the pressure drop.

CHAIRMAN BANERJEE: So the two are related. Right?

MR. KROTIUK: There is a relationship between the two, yes.

CHAIRMAN BANERJEE: Let's see where you go.

MR. KROTIUK: Okay. Next, wanted to --

CHAIRMAN BANERJEE: What happened to the pressure in this case?

MR. KROTIUK: That's the pressure here.

CHAIRMAN BANERJEE: Oh, it's that one. All right.

MR. KROTIUK: Okay. Now I'm just going to show three cases, one, two, three, four NUKON cal sil beds. Again, this is similar type of plot, as indicated previously, where I'm trying to give some numerical indication --

MR. WALLIS: Your maximum errors, you've got the error is five times the data, or six times.

MR. KROTIUK: Yes.

MR. WALLIS: Or .03 of the data, or something.

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: And I guess, you know, you feel comfortable about it, because none of these numbers is negative.

MR. KROTIUK: It's a --

MR. ABDEL-KHALIK: Not on the maximum end of it.

MR. KROTIUK: Oh, on the maximum end.

MR. WALLIS: Very small on negative. There are some negative --

CHAIRMAN BANERJEE: Well, they're averages.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Well, there is one maximum with a slight negative.

MR. KROTIUK: I'm going to show a plot here for a negative case, this one I'm showing, so you'll see that. Okay? And I have one for 1.5, a minus .4, and a .2.

CHAIRMAN BANERJEE: What about that minus .466?

MR. ABDEL-KHALIK: The next column.

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: I didn't plot that case. It's in the report, the plots of all these cases are in the NUREG. I just chose three cases to show here.

CHAIRMAN BANERJEE: Okay.

MR. ABDEL-KHALIK: Now the hand-out, two columns above, two rows above that gives us minus .695. Your slide doesn't show the negative sign, or is it just a bad slide?

MR. WALLIS: It's a bad slide. Minus is --

MR. ABDEL-KHALIK: One, two, three, four rows from the bottom.

MR. KROTIUK: Four rows from the bottom, minus .695, that should be -- gosh, I don't know what - it's a bad --

MR. ABDEL-KHALIK: Okay.

MR. KROTIUK: What's on the hard copy is correct.

MR. ABDEL-KHALIK: Okay.

MR. KROTIUK: Because I'm looking here and I could see a minus .695. I haven't been looking there.

MR. ABDEL-KHALIK: Okay.

MR. KROTIUK: I don't know what --

MR. WALLIS: I learned from this that you have a hard time coming close to the data.

MR. KROTIUK: That's right. Now let's look at the first case of comparison, that I'm comparing. And in this case, the data is down here. This is for a NUKON cal sil concentration of .6 for the cal sil, .13 kilograms per meter squared for the -- I'm sorry, .6 for the NUKON, and .13 kilogram meter squared for the cal sil. Again, it's around 20 degrees C, and in this case, I am over-predicting the test data, so my definition of --

MR. WALLIS: How did you do on the point where they had 1,000 times the homogeneous value, the extreme case in the small test loop, when they put in the cal sil, and then they put in the NUKON.

MR. KROTIUK: Oh, I couldn't --

MR. WALLIS: They got stuff which was way up there.

MR. KROTIUK: I could not do that calculation.

MR. WALLIS: Miles up there.

MR. KROTIUK: Unfortunately, that calculation could not be done, because I did not have the masses of the NUKON and the cal sil in the bed.

MR. WALLIS: You didn't report them?

MR. KROTIUK: That was not measured.

MR. WALLIS: They measured what they put in, but not --

MR. KROTIUK: They measured what they put in, but not what was in the bed.

MR. WALLIS: Okay. So you could bound it by assuming it's all in the bed.

MR. KROTIUK: I did not do that.

MR. WALLIS: I don't think you could predict that very, very high value. It doesn't look like it, because your two-volume model is never way up there.

MR. KROTIUK: I did not do that calculation.

CHAIRMAN BANERJEE: Strange, NUREG/CR-6224, all this seems to be coming in lower in prediction, and why is that?

MR. KROTIUK: One of the concerns that I have is that when the NUREG 6224 was developed, it always used in its calculations the mass of the debris added to the loop. It never measured what was on the debris bed.

MR. WALLIS: But then you'd expect that it would be higher.

MR. KROTIUK: So that if you use the mass data to the loop, it would raise the NUREG 6224 number.

MR. ABDEL-KHALIK: No, it would raise your data by --

MR. KROTIUK: It would raise my -- but my model is developed on the mass of the bed, not what was added to the loop. And for these curves, all these curves, I'm using the mass in the debris bed.

MR. ABDEL-KHALIK: Oh, I see.

CHAIRMAN BANERJEE: Including the NUREG/CR.

MR. KROTIUK: Including the NUREG/CR.

CHAIRMAN BANERJEE: Okay. That probably explains it.

MR. KROTIUK: Yes. So that was developed for mass added to the test loop for NUREG/CR.

CHAIRMAN BANERJEE: While we had our objections to NUREG/CR, we didn't think it would be that non-conservative. All right.

MR. KROTIUK: Okay? And the other thing I just want to point out here, is if I use that homogeneous one-volume model, it is at the lower limit of the test data.

CHAIRMAN BANERJEE: What's the difference between the homogeneous one-volume - I guess that's the difference.

MR. KROTIUK: That's the difference.

CHAIRMAN BANERJEE: Yes. But if you now use the total mass of the NUREG/CR, would it come up to the homogeneous one-volume calculation?

MR. KROTIUK: Yes. I actually did that calculation, and it comes up - some of the -- I did it for a number of them, and I don't remember for this one particularly, but it came up slightly below, or slightly above, but in the range of the one-volume calculation.

CHAIRMAN BANERJEE: In the cases that you don't have the mass on the --

MR. KROTIUK: Debris bed?

CHAIRMAN BANERJEE: -- debris bed, how will you use your model?

MR. KROTIUK: You have -- to my mind, the way you have to do that is that you have to develop a way of estimating how much is in the debris bed.

CHAIRMAN BANERJEE: How will you do that?

MR. KROTIUK: Well, if I add -- if I have a sump and I'm adding 1,000, whatever, hundreds of pounds of debris in there, you can't -- is it practical to assume that everything is on the screen?

CHAIRMAN BANERJEE: No, no, but in this case you've got a horizontal screen.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: So everything which doesn't pass through the screen and get taken away somewhere else, ends up on that screen.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: So even then you see quite a bit of mass missing. Right?

MR. KROTIUK: If I was trying to predict something from the test loop setup, I would assume that some high number, like 95 percent of the NUKON reaches the screen, and some lower number, which I'd have to look at the test data, some percentage of cal sil would reach the screen, and be deposited there. I mean, I would look at the test data and try to come up with some way of predicting that.

MR. WALLIS: Doesn't the cal sil go on being deposited forever? It keeps going around the loop, and every time you get some more of it deposited?

MR. KROTIUK: That's why in the test loop we put in the bypass filtration system. It tried to eliminate that.

MR. WALLIS: So in reality, you might as well assume it's all there. Eventually, it's going to get there.

MR. KROTIUK: Well, some of it --

CHAIRMAN BANERJEE: All of it that is entrained.

MR. KROTIUK: Yes. Some of it, don't forget, will pass through, also.

CHAIRMAN BANERJEE: Well, ultimately, it won't. Right?

MR. WALLIS: I can't imagine it passing through forever. Eventually it's going to take a path where it gets stopped. It's going to go into a part of the bed where it can't get through.

MR. KROTIUK: I don't know if I agree with that totally, because I think that particles may start moving through the bed, and eventually come out the other end.

CHAIRMAN BANERJEE: But you didn't do an experiment where you didn't remove the particles.

MR. TRAGONING: This is Rob Tragoning, Office of Research. Yes, we -- some of the earliest LANL tests, which there was -- which you all have seen, and there were issues associated with those tests, but some of the earliest thin bed effect tests were run exactly that way, where they continued to circulate the loop. And they showed exactly the phenomena that you might expect, that initially, the head loss build-up would be relatively small, but then you could reach a state where your filtration efficiency had increased due to the accumulation of larger size particles within the fibrous bed, such that it could trap some of the smaller ones. And then you could get situations where the head loss would elevate quite dramatically, and relatively quickly. But, again, we recognized at the time that that was an artifact of the fact that we were recirculating within that loop multiple times. That's one of the reasons within these tests that we wanted to run some tests where we only had a single bypass of the fluid. There were other tests that were done at PNNL where they did do multiple recirculation, but it wasn't the lion's share. And one of the things we wanted to realize is we weren't trying to simulate resonance times within these loops compared to what they would be like in an actual plant environment, because resonance times would be totally different.

CHAIRMAN BANERJEE: We're trying to understand how to use your correlation relationships, so should it be that given enough time, you have to assume that all the cal sil which hasn't dropped out, the live particles are going to be trapped on the filters?

MR. KROTIUK: I don't think I could answer that question right now. I have to think on that.

CHAIRMAN BANERJEE: Well, I mean, you have long-term recirculation tests both at Los Alamos, and at PNNL. Right?

MR. KROTIUK: We have fairly long recirculation.

CHAIRMAN BANERJEE: Yes, without -- you have some data without taking out the --

MR. KROTIUK: Without the bypass, yes.

CHAIRMAN BANERJEE: Okay. Now in that situation, of course, because it's at least in the PNNL, it's a vertical system, you would take out, I mean, all the fluids passing through that. Right? And there is no place to -- well, I suppose there are places to deposit particles --

MR. KROTIUK: One of the things - Tom, correct me if I'm wrong about this - I seem to remember when we were running the NUKON cal sil tests, when the cal sil was -- when we didn't use the filtration, so it was basically the Series-1 tests, that cal sil always seemed to pass through, and was recirculating during the entire test. Do you remember that?

CHAIRMAN BANERJEE: Forever?

MR. MICHENER: I can't recall exactly, but I remember that it was very dramatic when it would come through.

MR. KROTIUK: Because I remember that blob of cal sil.

MR. MICHENER: You'd see a cloud come through.

MR. KROTIUK: You could physically see the cloud.

MR. MICHENER: And then it would be clear again, and then a little while here would come that cloud again.

MR. KROTIUK: Yes, and it would continually do it during the entire length of the test.

CHAIRMAN BANERJEE: So it passed through the bed and come out?

MR. KROTIUK: That's what I was saying, I really think that a certain amount does pass through the bed. Because I do remember seeing the burps of --

MR. MICHENER: We were convinced that you had to filter, and that pretty much answers the question there.

CHAIRMAN BANERJEE: So if you had to use your model, what would you do about the mass that you would assume to be on the filter?

MR. KROTIUK: Well, as I said, in the NUKON, I would assume somewhere --

CHAIRMAN BANERJEE: Almost all of it.

MR. KROTIUK: All of it, right.

CHAIRMAN BANERJEE: It doesn't drop off.

MR. KROTIUK: And cal sil, I looked at the test data and see how much deposit would come up with a number - some way to assume --

CHAIRMAN BANERJEE: Which test data? Is the data --

MR. KROTIUK: All the test data.

CHAIRMAN BANERJEE: Do you have a recommendation on that?

MR. KROTIUK: I have not thought about that, so I don't know for the cal sil. And for the NUKON it's kind of obvious, for the cal sil, it's --

CHAIRMAN BANERJEE: Well, let's assume for the NUKON it's 100 percent.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Whatever is entrained goes there, but what happens with the cal sil is more tricky. Right?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Because these experiments that were done were taking out the fine particles, and the question then becomes, is there a big difference between when you take out the fine particles, and when you don't. You did some experiments without taking them out.

MR. KROTIUK: Yes, we did.

MR. WALLIS: Presumably, the fine particles have a different coefficient of everything than the coarse ones. Different surface area, value and everything.

CHAIRMAN BANERJEE: There's even the issue as to whether your correlation numbers like 300,000 really work.

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: Because if some fine particles get trapped, as Graham said, they have a higher surface area to volume ratio.

MR. KROTIUK: Well, I didn't think about that, and I don't have an answer on that right now.

CHAIRMAN BANERJEE: Okay. Well, let's continue with this.

MR. KROTIUK: This is for that test, the NUKON cal sil test, this is the comparison of the data which is in black, two-volume model prediction.

MR. WALLIS: Why doesn't NUREG always predict higher starting volume? I mean, you ought to know the velocity, the volume of the stuff when there's no velocity. You ought to know that, and yet they seem to be always off by a factor of 2 or something.

CHAIRMAN BANERJEE: I guess it's the artifact of what you assume is on the --

MR. KROTIUK: This is also -- you have to look at the NUREG correlation and look at the assumptions for densities, what they call theoretical or mac density, you know, insulation --

MR. WALLIS: You have taken the stuff and formed a bed, so presumably you've compressed it a bit more than if you just drop it in there.

MR. KROTIUK: I don't feel that I should in any way comment on the way that the NUREG is doing its calculation. This is applying it

THE WITNESS:

CHAIRMAN BANERJEE: But you're not applying it in the same way that they applied it.

MR. KROTIUK: The only difference --

CHAIRMAN BANERJEE: You take the total mass.

MR. KROTIUK: That's the only difference.

CHAIRMAN BANERJEE: Well, that's a big difference, though, isn't it?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: I mean, if you took

the fraction of the -- what would happen if you took the total mass?

MR. WALLIS: The velocity --

CHAIRMAN BANERJEE: Would it make it much thicker, or what?

MR. WALLIS: The velocity you'd form the bed.

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: Make it thicker.

MR. WALLIS: At what velocity do you form the bed?

MR. KROTIUK: This is formed at .1 feet per second.

MR. WALLIS: So you should have about the same thickness as they do, as you do. You've already, when you formed it, compressed down that green curve to whatever the point is there. You're not going to uncompress that to .6, you're going to go along at .3. So how you form the bed makes a difference. What you do here, if you form the bed at .01 or something, you might well be up on the NUREG curve. Once you've got this point --

MR. KROTIUK: There's other limitations. I don't really feel I should comment on the NUREG.

CHAIRMAN BANERJEE: All right. So you're

at .1, that's where your fixed point is. Right?

MR. KROTIUK: Right. That's my initial thickness.

CHAIRMAN BANERJEE: Right.

MR. ABDEL-KHALIK: Does the fact that your, in this particular case, the data and predictions for the height are close, is that totally fortuitous, or is that related to this sort of exponential fit of height that you did earlier?

MR. KROTIUK: In actuality, I studied that somewhat closely, and it's not related to that exponential delta M height. It's more related to the whole compressibility and expansion relations. And the initial calculation of bed thickness.

MR. ABDEL-KHALIK: Because the model seems to do better, I guess, for the two-volume in terms of height for the two-volume model than the single layer model.

MR. KROTIUK: I think you have to --

MR. ABDEL-KHALIK: And I was wondering --

MR. KROTIUK: I think you have to look at all the --

MR. ABDEL-KHALIK: -- if that's just artificial.

MR. KROTIUK: I think you have to look at all the plots, and it just happened - you can't make that generalization without looking at all of them. And, generally, I found that the match-up between the test data and the predictions for the model for both the NUKON and the NUKON cal sil tests --

MR. WALLIS: Well, can we look at some more figures, and keep going here?

MR. KROTIUK: This is another case.

MR. WALLIS: Where the data are close to the two-volume model.

MR. KROTIUK: And so this is, to my mind, saying that this is now a practical upper limit.

MR. WALLIS: And that's what should be used to be conservative?

MR. KROTIUK: Yes.

MR. WALLIS: And it's about 10 times what NUREG/CR-6224 would predict, something like that?

MR. KROTIUK: Using the mass on the bed.

CHAIRMAN BANERJEE: I mean, how much mass was -- what fraction of the cal sil was on the bed?

MR. KROTIUK: It's on top here. It's .243 NUKON, and .018 cal sil.

MR. WALLIS: There's not much cal sil.

CHAIRMAN BANERJEE: How much cal sil is it? What fraction is it?

MR. KROTIUK: I have fraction - I'm trying to do it in my head.

CHAIRMAN BANERJEE: Only 7 or 8 percent is in the bed. If you then use the NUREG the way it was, I mean, if you take all the cal sil and you bump it up somewhere to the same region, right?

MR. KROTIUK: Quite possibly. I don't remember this calculation, specifically.

MR. WALLIS: It seems strange that they've got a tiny bit of cal sil, but if you put it in the two-volume model, it has a very large effect on the pressure drop.

MR. KROTIUK: Right, because --

MR. WALLIS: That's where your exponential thing comes in, isn't it?

MR. KROTIUK: Actuality, this is down pretty low on the curve towards the zero.

CHAIRMAN BANERJEE: Well, I guess that reflects the physics. I mean, even if you --

MR. KROTIUK: You have a thin layer of cal sil.

CHAIRMAN BANERJEE: Yes.

MR. KROTIUK: And then this is the comparison of the bed thicknesses for that same test. And then, finally, I wanted to show one that I didn't get good match-up.

CHAIRMAN BANERJEE: That's the negative one.

MR. KROTIUK: And the model was not conservative, even. And there are, as you saw from that table, there are a few of them. And in this case, I'm under-predicting the test data.

CHAIRMAN BANERJEE: What fraction of the cal sil here is on the - again, how much cal sil? It could be that your measurement of the cal sil has dropped.

MR. WALLIS: Yes, there's a tiny, tiny bit of cal sil. We're down to .005.

CHAIRMAN BANERJEE: It could be that that's not a very accurate measurement. Right?

MR. KROTIUK: We measured the values as accurately as we could.

CHAIRMAN BANERJEE: Well, one thing could be to look at the two-volume model in the context of all the data, and see what fraction of the cal sil must be there to be always conservative.

MR. KROTIUK: What I found out when I looked at all the data is that the errors, the greatest error existed for the thinnest of that delta M max, the saturated thickness. And there seems to be a very strong dependency, so if you were down on that curve, if you were closer to zero, a small error in that saturated thickness could produce - a small difference in the saturated thickness could produce a big difference in the pressure drop; whereas, if you're out at the other end, small error in the calculated thickness would not affect things as greatly.

CHAIRMAN BANERJEE: And conversely, if you just say a small error in the cal sil measurement, because these are all measured values by difference. Right?

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: How sensitive are the results to that? You could be off by easily 100 percent on your cal sil measurement.

MR. KROTIUK: You know, that's a good -- I didn't look at that, because on the measurements for the NUKON and the cal sil, when everything was measured, we did have a plus or minus on it, and I took the medium value to do all these calculations. But in retrospect, I should have looked at some of the extremes to see if that could explain --

CHAIRMAN BANERJEE: Well, you're getting the cal sil by difference, right?

MR. KROTIUK: Right. Yes. But, I mean, PNNL did a whole statistical study to come up with the probability, the accuracy of those measurements, so I have - for all the numbers, I do have plus or minus values.

CHAIRMAN BANERJEE: Typically, what is the error on this, this very small number here?

MR. KROTIUK: You know, I don't --

MR. MICHENER: It seems like this was one of the areas that we thought we could improve on.

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Because it goes through your whole analysis. Right? I mean, in a way, you go through the uncertainty analysis, it would be interesting to see how it is affecting these numbers.

MR. KROTIUK: I just didn't do the calculations. It would have been interesting.

CHAIRMAN BANERJEE: Is this project finished right now? What's the status?

MR. KROTIUK: When I finish this presentation today, it's finished.

MR. WALLIS: So you aren't going to work on it any more?

MR. KROTIUK: Right.

CHAIRMAN BANERJEE: Now how is this going to be used?

MR. KROTIUK: I can't address that. Someone from NRR would have to address it.

CHAIRMAN BANERJEE: NRR needs some more analysis done, or some follow-up work in order to make this stuff applicable.

MR. WALLIS: It doesn't apply to a real thing. I mean, this is applied to a bed which is uniform, and horizontal, and everything. It doesn't apply to the typical screens that are actually used in a sump. It may give them some clue about things to look for, but they're going to use industrial data. I would be very surprised if they use this --

CHAIRMAN BANERJEE: At one point you have NUREG/CR-6224 was what was planned to be used. Right?

MR. LEHNING: This is John Lehning in NRR. And as you said, I guess in the safety evaluation that we wrote on the NEI guidance report for doing those sump designs, we requested that licensees do testing, rather than use the 6224 data, in light of some of the points that were raised about that correlation. And we did, and I guess in that paper, I think Mike Scott handed out to you all earlier, we did talk a little bit about their intended regulatory usage of this correlation. And as Dr. Wallis suggested, that in part due to some of the data limitations, that just - the only data that we had, this new correlation for it is NUKON and cal sil, and some of the other differences with the geometries and things like that. We were going to probably rely on testing, and showing the typicality and prototypicality of those tests.

MR. WALLIS: It does support your not using the 6224, because some of these data are quite a long way from that, so it may support that decision that you made.

MR. LEHNING: I guess, there are different parameters that can be put into that 6224 correlation. Some of the things with the density, as opposed to when you have blended fiber, as opposed to as-fabricated, and some of the other things out there. But, again, I would agree with that, that some of the things that you can clearly see that the 6224 would under-predict that.

MR. WALLIS: Can we move on to the temperature effects?

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: Just one more question. Of the 150 or so experiments that you ran, what is the fraction of those experiments in which the model under-predicts the data? I can't tell by max, min, and average. I can't tell by that.

MR. KROTIUK: I knew that number off-hand, and I've forgotten it. I forgot the answer to that, but I do remember that it was maybe something, 30, 40 percent. Something - I don't - don't hold me to the exact number, but it was a measurable amount. I just don't remember.

MR. WALLIS: This is when you use the two layers.

MR. KROTIUK: Two layers, right.

MR. ABDEL-KHALIK: Well, either way, I guess.

MR. KROTIUK: No, for the NUKON, it was -- for the NUKON only, the correlation is fine. That was for the NUKON cal sil.

MR. WALLIS: The two layers, which is the worst case you can think of, under-predicts the data, then something else is happening.

CHAIRMAN BANERJEE: And it's not the worst two layer model. The worst two layer model would be all cal sil.

MR. WALLIS: He's got that weird exponential thing. We don't know if that's worse or better than all cal sil.

CHAIRMAN BANERJEE: That's true.

MR. WALLIS: Can we look at the temperature sensitivity, because that sort of shows that you're not always --

MR. KROTIUK: Now this is for the NUKON only tests, and it's for the three - this is for the plot facsimile that I had before, and this shows, for instance, at 22-1/2 - about 20 degrees C, the calculation is up here, the data is down here. At 55, the calculation of data are about the same, and at around 80 degrees, the calculation is above the data.

MR. WALLIS: Then the next figure is --

MR. KROTIUK: Yes, so this is for the NUKON only. The NUKON only basically always gave this type of result.

MR. ABDEL-KHALIK: So temperature manifests itself in this model through viscosity?

MR. KROTIUK: Yes.

MR. ABDEL-KHALIK: That's the only place where it comes in.

MR. KROTIUK: That's the primary -- yes. I mean, we postulated that there could be some other effects, like possibly the NUKON becoming more flexible, and --

MR. ABDEL-KHALIK: No, I'm thinking about the model, the prediction --

MR. KROTIUK: Yes, for the model --

MR. ABDEL-KHALIK: It only comes in through viscosity.

MR. KROTIUK: Okay. Now this is for a NUKON cal sil case, showing the comparisons. This is the data -- this is the one that I showed that - previously on the previous one, where the calculation was actually below test data. But then we went up to 55 degrees or so, the calculation and the test data --

MR. WALLIS: It's weird that the calculation is -- I mean, the trend with viscosity is okay for the data, but not for the calculation.

MR. KROTIUK: This is NUKON cal sil, so that's why I said before, that the trend for NUKON only was directly related to the viscosity. For the NUKON cal sil, there's other stuff there.

MR. WALLIS: Other stuff besides viscosity?

MR. KROTIUK: Well, there's other considerations.

MR. WALLIS: Such as?

MR. KROTIUK: Such as possibly redistribution of material.

MR. WALLIS: But your calculation should

have a consistent trend.

MR. KROTIUK: Yes, but if you remember, this was one of the -- this could be one of the cases where the model is under-predicting the data. I said that there was --

MR. WALLIS: AT least a trend should be there, the calculation should show increasing pressure drop with increasing viscosity, and it doesn't.

MR. KROTIUK: Yes.

MR. WALLIS: It's strange.

MR. KROTIUK: Yes.

MR. MICHENER: Tom Michener from PNNL. This is the one that's the strangest here, the 20 degree one, that's one of the cases where the cal sil was measured very, very low, so that may be --

MR. WALLIS: Or it's something else. It's not just the temperature.

MR. MICHENER: So I point that out, that is that one.

MR. WALLIS: Not just the temperature effect.

MR. KROTIUK: Then, finally, this is for --

MR. WALLIS: That's also very reassuring, isn't it?

MR. KROTIUK: This was the case where we had the measurement show that the -- the data showed that the delta P measurement for the higher temperature was actually higher for the lower than temperature, which is -- whereas, the calculation is showing that --

MR. WALLIS: So can we look at the conclusions, and then see where we are?

MR. KROTIUK: Okay. As I said, for the one-volume homogeneous model, I did comparisons, and they are in the NUREG for PNNL, ANL, and LANL tests. They were all, the comparisons are all in there. And generally for the NUKON only tests, the model does predict conservative results for all the NUKON only tests. For the NUKON cal sil test, it generally predicts - not generally, it always predicted pressure drops that were at or below the measurements.

MR. WALLIS: Always?

MR. KROTIUK: For the data, for the comparisons of the --

MR. ABDEL-KHALIK: This is the one-volume.

MR. WALLIS: One-volume.

MR. KROTIUK: One-volume, right. For the two-volume model, the model gave good comparative or conservatively higher results for all the tests where the cal sil saturation thickness was thicker relative to the entire bed. When the calculated thickness, or the saturation thickness was small, as I said, the small inaccuracies in that calculated thickness could result in large differences in calculation of pressure drop, so there could have been - there could be little differences between the measurements and the predictions. And as I said before, that calculation of that saturation or upper limit thickness is really the thing that I feel could do with some improvement.

CHAIRMAN BANERJEE: Is it necessary to -- I mean, couldn't you just bound it by separating the two layers?

MR. KROTIUK: That's a thought, and I didn't do that calculation, so I don't know. I may try that just out of curiosity.

MR. WALLIS: You wouldn't need any exponential stuff.

MR. KROTIUK: Right. I'm curious to see what happens with that.

MR. WALLIS: It's a very simple thing to do.

MR. KROTIUK: Yes.

MR. WALLIS: I mean you could say that always bounds everything. It might be a candidate for a conservative analysis.

MR. KROTIUK: Well, I'll take a look at that.

MR. WALLIS: It seems to me that you have allowed now a new degree of freedom, which says you can have homogeneous layers. And since you put in a new degree of freedom, you'd expect to be able to predict data better because you've got a new degree of freedom to fit things, and so on. Which is probably something like reality, there probably are two layers, or there's a gradient of cal sil or something, so you put in some reasonable physics that catches some of the major things going on. I think this has got a way to go before it's a predictive tool.

CHAIRMAN BANERJEE: In any case, it would have to be married to some sort of a CSD tool, to estimate locally what the concentrations are, what has dropped out and what hasn't. Otherwise, how would --

MR. KROTIUK: I don't know if you really want to do that. I mean, my approach to this was trying to develop something that would be upper and lower limits. Do you really need to know --

MR. WALLIS: But you don't do that. You want to predict that the whole screen is covered with a thin layer of cal sil. That's terrible.

CHAIRMAN BANERJEE: That would be -- I mean, the practical problems are often, as you know, that you have multiple top hats or something behind each other, so maybe the first few of them pick up a lot of it, and then some of the later ones don't have some. It's hard to -- I don't know what NRR's intentions are, but it's very hard to see how industry can do tests to cover all these eventualities without some sort of a tool to interpret these experiments and bring them to full scale. I mean, it would be nice to be enlightened at one point about that.

MR. KROTIUK: Right. And just the final conclusion, is that the bed thickness predictions, in my opinion, were pretty close at least to trending for all the cases modeled, and that the -- generally, the method, calculation method predict higher pressure drops at the lower temperatures, which is consistent with the classical theory. However, it could be - when compared to test data, it could be affected by various things, flows, temperatures that affect distribution of material in the bed.

CHAIRMAN BANERJEE: Is that it?

MR. KROTIUK: That's it.

CHAIRMAN BANERJEE: Thank you. I think we kept you for a long time, but if there are some more questions - anybody wants to --

MR. XIAO: Dr. Banerjee, if I may - Tony Xiao from Research. I want to thank you for this opportunity to come in front of your committee to basically do a wrap-up of the several of the projects you may not have heard before. Earlier this - we want to thank you for your input, as well. You provided some recommendations and questions that made us go back to rethink, and maybe things we could do better, that we should do better.

I just want to assure you that we are not closing shop from this point on. We will continue to closely work with the NRR staff, and industry, and our staff will monitor industry's input and how they implement methods to resolve the GSI-191 issue at their plants. If necessary, we'll get into agreement with NRR, we'll do some more very defined, small-scale research in the future, if necessary.

As far as for the scheduled next week's full committee, at this moment, I don't see we have new information between now and then to come in front of the full committee, so I ask your decision for the full committee, whether we will have to come back and brief the full committee on the same topic, using pretty much the same material. But certainly, if we do come back, we will take today's lessons learned and try to prepare better, and answer some of the questions, if possible. But, by and large, we will not have new information between now and then, and we are not asking the committee to provide a letter to us at this time.

MR. WALLIS: So you would be -- this is more of an informative thing where you tell the committee what's been going on, but you're not ready for a letter.

MR. XIAO: Right, we're not asking the committee.

CHAIRMAN BANERJEE: Is this an item on the committee's agenda?

MR. CARUSO: Yes, it is.

MR. WALLIS: So we have to do it.

CHAIRMAN BANERJEE: How much time do you have?

MR. CARUSO: I don't have it here. I would say I think two hours.

MR. WALLIS: Two hours?

MR. CARUSO: I think this is a two-hour item.

CHAIRMAN BANERJEE: And at the moment, they're not asking for any response, it's only for information.

MR. CARUSO: It's up to us.

CHAIRMAN BANERJEE: Yes, up to us. Now the second thing is, there is some plans for -- thank you, Dr. Krotiuk. I didn't mean to leave you sitting there.

MR. KROTIUK: Yes, I was going to leave.

CHAIRMAN BANERJEE: Yes, it was very helpful.

MR. WALLIS: Don't go away.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: Even though it's 5:00.

MR. KROTIUK: Okay.

CHAIRMAN BANERJEE: Is there plans for NRR to come to the committee or the subcommittee?

MR. CARUSO: There are some plans, tentative plans to have NRR come to talk to the subcommittee in late May.

CHAIRMAN BANERJEE: Okay.

MR. SCOTT: And we have - Mike Scott, NRR. We have a number of subjects that we'd be interested in talking to you about. For example, we will have done several audits by then, we can come in and talk about that. And we are also making plans for the generic letter response reviews, we can talk about that. Maybe that would be of limited interest to you. We may by May have some indications of the testing that's planned and ongoing by the industry. As I mentioned earlier, there's one utility, at least, that's finished their chemical testing, so hopefully by May, there will be several, so we can come in and tell you what we know at that point about what's going on with the testing.

We'll also be able to talk to you about progress on the downstream effects ex-vessel topical report, so there's several things that we'll be ready to talk to you about, at least give you a progress report on then.

CHAIRMAN BANERJEE: Would you want to come, I guess, in front of the subcommittee, initially, and then in front of the full committee?

MR. SCOTT: I think that was the idea. I think Ralph and I went back and forth about a date. He said late May, I think my preference was middle.

MR. CARUSO: Middle May.

MR. SCOTT: Middle May, yes. I'm going to be out the last week in May.

CHAIRMAN BANERJEE: All right.

MR. WALLIS: Well, in front of the full committee, what are you going to say? It seems to me you have to make some key decisions. One is, whether you're only going to discuss new material, because they have heard lots of this before, or whether you're going to also take a look at what you've learned from all of these tests, which are in these NUREGs that are coming out, and make some sort of summary of what's the state-of-the-art that you've established, that you didn't do today.

CHAIRMAN BANERJEE: I guess what -- if I heard Mike correctly, he was saying that you have some results of audits, and some of the industry efforts, and programs --

MR. WALLIS: But NRR isn't on our program, are they?

CHAIRMAN BANERJEE: In May, I'm saying. I'm talking about the May and June meeting.

MR. WALLIS: Oh, you're talking about May.

MR. SCOTT: Okay. Yes, I was --

CHAIRMAN BANERJEE: Talking about the May meeting really right now. So for next week's meeting, all we can do is discuss what was done today.

MR. WALLIS: Is that right?

CHAIRMAN BANERJEE: We had two different things.

CHAIRMAN BANERJEE: But is that a

worthwhile use of the full committee time, to listen to this story that we heard today?

MR. CARUSO: Unfortunately, you're stuck, because the agenda has been published.

CHAIRMAN BANERJEE: I think the information would be useful, because when that is -- the information in May would be added.

MR. WALLIS: Well, you could certainly talk about the peer review.

MR. XIAO: We look to your direction. If you feel the full committee would benefit from hearing a condensed version, and we'll focus on the peer review, we will do that, so I'm just asking whether you think it's necessary for us to come back to do it again in a condensed version next week.

MR. WALLIS: You don't want to come back next week at all?

MR. XIAO: If you ask us to, we will.

MR. WALLIS: Well, I think Ralph is saying you're on the agenda. There's a performance by you scheduled, and it's been advertised, and you've got to show up.

MR. KRESS: You pretty much have to.

CHAIRMAN BANERJEE: You have to.

MR. XIAO: I thought maybe you can send

out an agenda change, if you feel it's not --

MR. KRESS: It's too late.

MR. XIAO: -- it will not benefit.

MR. KRESS: Even when we change it, we have to advertise them in the Federal Register.

MR. CARUSO: The only thing we can do is just have a big hole, go off and have a smoke.

MR. WALLIS: We had that before, we have sometimes had that, but very exceptionally.

CHAIRMAN BANERJEE: Well, I think we should assume for the time being that there will be a presentation, and get ideas from the subcommittee as to --

MR. WALLIS: To fill an hour?

CHAIRMAN BANERJEE: Well, I think how - I'm sure it will fill more than an hour, but how best to present the information so that we have the most usefulness, so maybe we could start with Tom, and ask him.

MR. KRESS: Well, I think I would focus on the peer review, because it's new, and the rest of the committee haven't heard it. As far as this discussion on the modeling and comparison of the data, it's all very interesting, but I don't think we have time for that, and it's - I don't think I would get into it.

I think I'd just focus on the peer review.

CHAIRMAN BANERJEE: What about you, Graham?

MR. WALLIS: Well, I think that maybe they could hear about what's new, rather than - although I would kind of like a perspective on what we've learned from the entire program, since you're wrapping up all this research, and you might sort of - but I don't think you're ready to do that, so what did we learn from all this work we've done?

Peer review I would think could take maybe 60 percent of the time or something, but then you've got to do it right. I mean, you've got to give an honest assessment of all these criticisms you've gotten so the 50 or so recommendations, and how you're going to respond to all that, I think you've got a lot of work to do to put together a presentation along those lines, because that's not really what we heard today.

MR. XIAO: Correct.

MR. WALLIS: I do think it's worth mentioning that there has been this new work on the Westinghouse surrogate. I mean, that struck me as being something that was important.

MR. KRESS: Yes, that's important.

MR. WALLIS: Realize that this stuff could really clog screens probably more effectively than some of the things that Argonne has been using.

CHAIRMAN BANERJEE: Well, equally, let's say.

MR. WALLIS: Well, it looked as if it was maybe even more effective, but I think some summary of that. And I wouldn't say nothing about the modeling effort. I think you could say that there's been this modeling effort. This is roughly - sketch out very briefly how it's being done, and show that it sometimes work, and it sometimes doesn't work, and probably conclude that it's not a tool which is ready for use.

MR. KRESS: They could show that slide with the four different --

MR. WALLIS: There are probably about five or six slides showing, here's the basis of the model, here is where it works, here's where it doesn't work, here's some of the anomalies, and I think you're going to conclude, probably, that it's not something that you can use, but you've learned a few things qualitatively, which might be useful for NRR in deciding their RAIs and so on.

MR. XIAO: Okay. If that's what you

prefer, we will come back and use next --

CHAIRMAN BANERJEE: Let's hear from Said.

MR. ABDEL-KHALIK: I would concur fully with what Dr. Wallis has said, particularly with regard to the peer review. I think a rehash of what was presented today would be not very useful.

MR. XIAO: I understand.

MR. ABDEL-KHALIK: I think a much more thoughtful assessment of the comments and recommendations made by the peer review, and how you plan to address those comments, and the rationale for your decision as to how you're going to address those comments would be much more valuable. As far as the other two items, I fully agree with what Dr. Wallis has said.

CHAIRMAN BANERJEE: And do you feel, Tom, that it would be important to present a very brief outline of the surrogate experiments?

MR. KRESS: Oh, yes. I really think that's very important.

CHAIRMAN BANERJEE: So I think then you have our feelings fairly clearly. The only thing is --

MR. WALLIS: We don't know what you think.

CHAIRMAN BANERJEE: Well, I probably said what I thought. But you've got two hours, let's say, so you would want to organize things so that roughly 60 to 70 percent of that time is taken up with analysis of the peer review and what actions and responses you have. And then the rest with the surrogate experiments, and maybe just an outline of the work you've done with the correlations. I think that's valuable, even if it's not immediately applicable. It's indicated some thoughts about separating these into two layers, and get a much better correlation with that, so I think that's useful. But you want to keep it short.

MR. XIAO: Yes. I saw Bill, as you were talking, as the other members are talking, I saw Bill jotting down his notes, so I'm glad he's still here to hear it from you again to emphasize that. We will --

CHAIRMAN BANERJEE: Fifty-nine slides, four or five. Right?

MR. XIAO: Okay. We'll be limited to four or five slides. And I also heard, very clear to me --

MR. WALLIS: Back-up slides, have some back-up slides, because it may well be that you're going to get a lot of discussion and questions which could be answered by a suitable back-up slide.

MR. XIAO: We'll prepare those, as well. We'll concentrate 60 to 70 percent on peer review.

MR. WALLIS: Very short, perhaps have a lot of substance that you can turn to if you need it.

MR. XIAO: Okay. We'll focus on peer review as to how we're going to address them, the rationale behind --

MR. WALLIS: Have you made up your mind? Now are you going to in a week decide how you're going to respond to the --

MR. XIAO: It's going to be a challenge, but it's a challenge going in front of the committee at any time, so we'll take that challenge and do better. I know we'll do better, how much better to your full satisfaction, we'll try.

MR. WALLIS: Because if you don't do it, then I may be tempted to quote from the peer review and say okay, here's a statement, how are you going to respond?

MR. XIAO: We'll try to prepare better so you don't have to quote.

MR. TRAGONING: Yes, but just to be realistic here, it's a relatively short time. I wouldn't expect disposition of the peer review comments between now and then. I think we'll certainly indicate path forward. Another thing that we can certainly do in this area is to prioritize and highlight some of the ones that the peer reviewers themselves thought were particularly important, and we have information to do that.

We can present some strategies for moving forward, but we may - and some of them, we may actually have a disposition, but I would say, by and large, we probably won't have that, most of that information in terms of the exact disposition of comment A, B, C, D by the next week or two.

CHAIRMAN BANERJEE: If you can get help from the PIRT to order your thinking, and present it in that form, that would be very useful.

MR. TRAGONING: Yes, and that's the plan. We're going to inform the slides by the PIRT process, and be able to, again, a little bit more systematically present issues that percolated up in the PIRT, as well.

CHAIRMAN BANERJEE: Okay.

MR. XIAO: We look forward to coming back next week.

CHAIRMAN BANERJEE: All right. Thank you, then. Do we have any more discussions?

MR. KRESS: Well, we probably ought to

have comments on what we've heard so far.

CHAIRMAN BANERJEE: Okay. So if you want to say, say it.

MR. KRESS: Okay. Well, all in all, the --I thought it was a pretty good wrap-up. One thing that bothers me is I'm disappointed that we haven't made more use of chemical equilibrium models. I was under the impression that you should be able to bound the kinetics effects with these, and I haven't seen any evidence of that, so that's one problem I have.

I still think there's a need for an overall integral predictive model which would include these chemical equilibrium. And I think that should be the reason for doing additional research, to pull that together, and I think it will be needed, as confirmatory to their judgments they're passing on the adequacy of the plant-specific tests. That's where I think it's going to be needed.

I think the peer reviewers did a good job, and I agree that there's a real need to respond to each and every one of their comments, not necessarily to agree with them, but to respond to them.

I thought the modeling approach was interesting and promising, that I still consider a bit of a work in progress. I think the curve exponential relating the percent cal thickness to the percent cal mass needs a little rethinking. And I think you need to figure out, like Sanjoy said, how to apply this to real screens that may fill up in a non-uniform manner, so you need - I mean, you don't just have a screen with two layers on it, you have lots of strains with different relations to these through it. And so, I think there's a need to apply it to the real world. Other than that, I thought it was a pretty good day.

MR. WALLIS: What I miss is some leadership for all this research. I see a lot of projects which are sort of not complete, and they've discovered some things, and other things haven't been, and so on. I would like to have someone knowledgeable, and that's really what a manager should be able to do, a technical manager should say these are the things we have established by this research, and these are things which we need to do, or what somebody needs to do, and so put the whole thing in some kind of technical perspective, and I really miss that. I mean, someone who's here who's on track, who's going to put the work in the best light, and so on.

And then the peer reviewers do some of this. They actually point to this here, well, this misses this and so on. I don't see a sort of a technical management perspective, where someone who's in charge of this whole effort, the NRC in a technical way, knows what's been achieved, and what hasn't been achieved, and whether or not it's met some objectives, or whether some other objectives have been neglected, and all that. That's something I really miss, and I don't think I'm going to get it, but I would really like to. Then I'd feel really competent that somebody here knew what was going on technically with this issue.

MR. SCOTT: If I can interject one point - one thing I would recommend you do is look at the document that we gave you tardily today, and it shows at least what NRR plans to do with the research results, take a look at that, maybe it gets you part way what you're interested in.

MR. WALLIS: Well, I understand the NRR perspective. I mean, I'm just asking the RES perspective on it. You do the best you can with this stuff, and you're trying to solve the problem, and you're going to rely a lot on industry, I understand.

MR. SCOTT: And I think maybe part of the answer to the research part, and I'm speaking for Tony here, is the RIL that you all were talking about putting together. Right? That will have that kind of perspective, will it not?

MR. XIAO: Yes, I think Rob - I wasn't here, but I believe Rob Tragoning had mentioned there is work on the RIL. It will be available later on this year, probably May/June time frame.

MR. WALLIS: This was to summarize the result and what the messages are from it, and so on.

MR. XIAO: Yes.

MR. WALLIS: That would be very good. I look forward to that.

MR. XIAO: Okay.

MR. ABDEL-KHALIK: I would like to echo that. There are just too many loose ends here. NRC is doing research, there is an independent peer review, industry is going to do work, and somehow all of this has to be connected to come up with a coherent useful story, where people in NRR can actually use it to their best advantage, rather than eliminating or excluding part of it, or relying to a large extent on only a part of it.

The second comment I would like to make is that with regard to the presentation that we heard towards the end of the day, a lot of effort has been expended on the experiments in the pressure drop modeling, and it would seem that with some modest additional effort in the modeling area, we can extract a lot more from the data that we already have. And I would sort of recommend that that not be stopped. It would just seem like the return on investment in that case would be quite significant. Just more though, modest effort really should go into analyzing the data, and coming up with a much more robust model. Those are my comments.

MR. WALLIS: But, Said, if you were running a research program yourself, and you wanted to write a proposal to solve this problem, or if I were doing it, I think I would need quite a few man-years.

MR. ABDEL-KHALIK: Well, what I'm trying to see is what is the best we can do with the information that we have with the relatively modest additional resources.

MR. WALLIS: The trouble with this problem is there keeping being surprises, and you sort of think you've got a correlation, and then someone does another experiment, and then it doesn't work, so it's very difficult to really comprehensively cover all eventualities.

MR. ABDEL-KHALIK: But the starting point really ought to be sort of a thoughtful assessment of the peer review comments. These are by and large people who have put in a lot of effort, and a lot of thought into coming up with their comments and their reports, and it would seem like we ought to take advantage of all that knowledge and wisdom that they have put forth, so the idea of organizing the response to the peer review, and coming up with why you accept or reject some of these comments, and how would you respond to them, would probably be --

MR. WALLIS: I guess what I was asking for, too, would be more of an internal peer review. And if you folks - a lot of the stuff the peer review people came up with, I would think that you guys would come up with on your own.

CHAIRMAN BANERJEE: Well, there were some things which apparently weren't thought of. First of all, I'd like to say that I very much appreciate and commend the staff for going out for such a thorough peer review. I've seen a lot of peer reviews, and this was a pretty good one. And they were serious people, they did a serious job, and to expose yourself to the extent that allows these people to do this, I think that's very commendable. And it's a first-rate thing to do.

Now I think it's simply having to deal with this, and learn the lessons from it, and go forward, as Rob said, to have a path forward. And I think the PIRT is a bit overdue, but once that is done, at least you'll have things prioritized and clear, clearer, let's put it.

The problem seems a very big problem, and I don't think that it's going to very easy, personally, to have some sort of a predictive model for the effects. What the research can do is to indicate directions that industry may or may not take to deal with it, which may simply be to circumvent the problem in some way by changing the buffering agents, or whatever. I mean, whatever information that can be made available to help that process would be useful. And things should be organized so that that can be done.

I do think, though, that the work on the head loss is going in the right direction, but it's still very much a work in progress, as somebody else said. And to really make it useful, it will need to be coupled to something which is a little bit more global. I said CFD, but it doesn't have to be CFD. There can be other ways of dealing with this, because my feeling about what the industry will do is, they'll do a series of tests, perhaps they'll do it in water tunnels with a screen at the end, and with some conditions which are typical, and look at maybe drop-out in front of the screen. I don't know exactly what they'll do. If I was in industry, that's what I would do, probably.

Now how do you interpret this, because the real situation is going to be very complicated, and the sump screens which will take out a lot of stuff, other parts of it it won't. There has to be some sort of a tool which can be used as a structure to interpret what the industry proof tests are, put it into a framework. And maybe the sort of work that's being done on the head loss modeling could be phrased into this structure even to interpret the sort of - what do these experiments mean? How do we interpret the models? Eventually, there's no escape from some form of modeling to scale up.

MR. WALLIS: I don't think there's any model for the performance of the industrial-type screens.

CHAIRMAN BANERJEE: Yes. Well, how do you operate in the absence of a model?

MR. WALLIS: They're not homogeneous, they're usually vertical, they have pockets in them, there's all kinds of stuff, and there's no model for that.

CHAIRMAN BANERJEE: Can be a very empirical one, but I don't know what direction that would take. But it concerns me to operate without some sort of a framework, and just depend on proof tests. Anyway, those are my comments, and hopefully, in May we'll know more about the approach industry is taking, and what actually they're accomplishing, so that would be an important meeting.

MR. SCOTT: If I can insert one more thing - we do now have one of our audit reports is public, and we'll, of course, get that over to Ralph. And when you all have a chance, you might want to take a look at that. And you'll get an idea from that as to what sort of things we're finding out there, and what we're writing up as an open item. And by May, we'll have at least two more audit reports available, so those will give you some perspective to support the May meeting, too.

MR. WALLIS: Include chemical effects?

MR. SCOTT: Well, unfortunately, what we're saying for chemical effects is, you haven't done it yet for those plants that we've evaluated. But as far as head loss and the other, many of the other subject areas, there's a greater degree of completion, so for those areas, you'll have perspective. For the chemical effects, what I would say we would plan to do is talk to you at that point about what we know about how the testing is going. We probably will not have an audit report in-hand at that time speaking to chemical effects.

MR. WALLIS: There are chemical effects, what use are head loss tests without chemical effects?

MR. SCOTT: Well, you learn information about their methodology as applied to the head loss testing. What they may have to do, and a lot of the plants will have to do, is redo the testing once chemicals are factored in.

MR. WALLIS: Or get rid of the chemical effects.

MR. SCOTT: Or get rid of the chemical effects, yes. And they can simplify their problem in many cases by that. And as we've said before, each plant is going to look at it from their own particular plant-specific situation, decide what combination of all the above measures makes sense for them. And the environment at any plant a year from now is going to be very different from the environment that it was two years ago when we started in all these mods.

MR. XIAO: This is Tony Xiao again from Research. I would like to say that is really a result of the research we have conducted for the past few years, that contributed to this kind of decision, the regulatory decision to help industry to at least take the right steps, steps in the right direction to avoid certain things that will help the situation.

MR. WALLIS: Now, Tony, you said you don't want a letter from the ACRS?

MR. XIAO: Correct.

MR. WALLIS: What is -- I think I was assigned to write a letter, draft letter. Was that right?

MR. CARUSO: I think so.

MR. WALLIS: I want to know - my inclination is not to draft a letter, but if the committee, subcommittee feels that a letter should be drafted, then I should do it. I'm hoping at the moment that we don't have to write a letter. I'm not quite sure how we add value in the most useful way by writing a letter at this stage.

MR. XIAO: Correct. Just a personal suggestion, I think I would suggest that probably a better time to write a letter is after May or June when NRR came back and gave you their status update, and we will come back, also describe the RIL we talked about earlier. That would be a better time.

MR. CARUSO: Also, that we have a meeting with the commission in June.

MR. WALLIS: We don't want to discuss sumps again.

CHAIRMAN BANERJEE: We might have to.

MR. CARUSO: They've always been asking for it, so that's just something to consider.

MR. SCOTT: Are you going to hang around for that, Ralph?

MR. CARUSO: I'm not.

MR. XIAO: On a personal note, starting next week, I have a new assignment. I'll be working at NRO, but I was trying to get my replacement here today to be part of this meeting, but she was not in. Her name is Rosemary Hogan. Some of you may know her. But I'll stick around for a couple of more weeks just to make sure we have good transition, and I will make sure she will be here next week for that meeting.

CHAIRMAN BANERJEE: But who is going to coordinate your presentations next week, you?

MR. XIAO: I will.

CHAIRMAN BANERJEE: Okay.

MR. XIAO: I will.

CHAIRMAN BANERJEE: Okay. If we have no other business --

MR. WALLIS: Then somebody new is going to be managing the program?

MR. XIAO: Rosemary Hogan will be the new Branch Chief.

MR. WALLIS: Will there be any continuity then?

MR. XIAO: Absolutely, there will be continuity.

MR. WALLIS: She'll have to be briefed on everything all over again, and --

MR. XIAO: Absolutely. Me and my staff will do that. I mean, and that's why I was hoping she would be here, get a taste of what kind of questions the ACRS may have, but she was not in. But next week, she will be here.

MR. KRESS: And where are you going?

MR. XIAO: NRO, Office of New Reactors.

MR. KRESS: Oh.

CHAIRMAN BANERJEE: Sump screens will be interesting with passive circulation.

MR. XIAO: We'll probably see you also in a different light.

MR. SCOTT: Regarding continuity on the NRR side, I have been told I can't go anywhere else until this is resolved, so you don't need to worry about that one.

MR. CARUSO: Until you retire.

MR. SCOTT: Or until I retire.

MR. WALLIS: What is the question - when we look at new reactors, like I supposed AP-1000 is a new reactor.

MR. SCOTT: What was your question?

MR. WALLIS: How does sump screen questions affect things like AP-1000, which is a new reactor, since Tony brought up the new reactors.

MR. SCOTT: We've been discussing that very subject, as a matter of fact. We looked at all of the new reactor designs, both the Bs and the Ps from the perspective of strainer clogging, and the situation for each one of them varies dramatically, depending on the time line involved. For example, ABWR was certified in 1994, and the BWR operating plant corrective actions were taken about two or three years after that, so we just sent, NRR just sent a memo to NRO identifying the disparity between where the ABWR was certified, and the rest for the other BWRs, and suggesting that they address that with General Electric, which I believe they plan to do.

AP-1000 was certified early on in the GSI-191 resolution process. There are a number of COL action items for AP-1000 that reflect sump concerns. I would say that we are smarter now than we were then, and so is Westinghouse, and so we understand that Westinghouse plans to submit a topical report on sumps to the NRC, among many topical reports, evidently, that they are submitting to address, I guess, the progress of knowledge since that design was certified.

ESBWR and EPR, of course, are either in current review, or not started review yet, and so we're fully up to speed, and involved with the reviews of those designs. Of course, that work scope is going to NRO, and some of the expertise will follow it so that they can do those reviews. So we're working all that.

MR. WALLIS: The ESBWR doesn't really have sumps, and it doesn't have places where debris gets into the tanks, which then inject water.

MR. SCOTT: They do have a strainer in there, I believe it's called the gravity-driven, yes, I'm not sure exactly.

MR. WALLIS: The PRA has strainers, and it has some estimate of whether or not the strainers blocked. I noticed that.

MR. SCOTT: Certainly, their vulnerabilities, or lack thereof, are very different from the current generation of plants. And the interesting thing is that these designs are so different, each of them, from the traditional designs that are out there now that you can't take your PWR guidance and just plug it into the AP-1000, or the same for the BWR guidance for the ESBWR. You have to look at it specific to that design. And in many cases, their vulnerability hopefully will be less, and they've taken a number of measures in each of these new reactors to reduce vulnerability. For example, the materials of construction for the BWRs, they've gone from carbon steel to stainless steel to minimize the amount of sludge they're going to have in their suppression pool, so there have been a lot of changes, but we're looking at that.

CHAIRMAN BANERJEE: Well, one thing would be to design out chemical effects.

MR. SCOTT: Absolutely. Well, another thing is to design out vulnerabilities. For example, I'm trying to recall which one of the designs it is - I think the ABWR may have committed to all RMI insulation, so the fiber is all gone.

MR. KRESS: The IRIS doesn't look like it would be vulnerable to this at all.

MR. SCOTT: No insulation of consequence.

Is that the issue?

MR. KRESS: Right.

MR. SCOTT: Yes.

MR. XIAO: The new reactor designs are like Dr. Banerjee argued, design out a lot of the issue, vulnerabilities. One is the sump issue, the other one is aircraft impact, so there's a lot of effort there, too. And I believe we're also coming back to the ACRS some time next week to give a summary of our plans to do the aircraft.

CHAIRMAN BANERJEE: Aircraft. All right. If there are no more discussion, then I'm going to adjourn this meeting until tomorrow.

MR. CARUSO: No, not tomorrow. We're going to adjourn today.

CHAIRMAN BANERJEE: Oh, adjourn today.

MR. CARUSO: Different meeting tomorrow.

CHAIRMAN BANERJEE: Oh, different meeting tomorrow. All right. Adjourn today.

(Whereupon, the proceedings went off the record at 5:35:17 p.m.)